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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

H. H. TURNER, M.A., B.Sc., F.R.A.S.,

AND

A. A. COMMON, LL.D., F.R.S., Treas. R.A.S.

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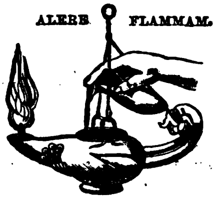
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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 170. 1891. Vol. XIV.

INTRODUCTION.

In the absence of any criticism or suggestions as to the contents and arrangement of last year's 'Companion,' we have made no change in the present issue.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," Mr. Lewis for the Double Stars, for some of which Mr. Maw has kindly supplied observations. M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations and phenomena of Jupiter's Satellites are from the 'Nautical Almanac'; the diagrams and Ephemerides of the Satellites of the other planets from the 'American Ephemeris.'

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign – indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used in all cases, and the astronomical day is reckoned from noon to noon as in previous years.

Date.	Day of Year.	Fr. of Year.	Sun Sets.	Sun Rises.	☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			h m	h m	° ′	h m s	h m s	d h m
Jan. 4	4	·008	4 2	20 8	22 44 S	0 5 8	18 55 14	☾ 2 22 12
11	11	·027	4 11	20 4	21 48	8 8	19 22 50	● 10 3 25
18	18	·047	4 22	19 58	20 33	10 38	19 50 25	☽ 16 18 18
25	25	·066	4 34	19 50	18 58	12 33	20 18 1	○ 24 12 25
Feb. 1	32	·085	4 47	19 40	17 6	13 48	20 45 37	☾ 1 16 42
8	39	·104	4 59	19 28	14 59	14 24	21 13 13	● 8 14 12
15	46	·123	5 12	19 15	12 40	14 22	21 40 49	☽ 15 6 30
22	53	·142	5 25	19 1	10 11	13 43	22 8 25	○ 23 7 18
Mar. 1	60	·162	5 37	18 46	7 35	12 33	22 36 1	☾ 3 7 37
8	67	·181	5 50	18 30	4 53	10 59	23 3 37	● 9 23 51
15	74	·200	6 2	18 14	2 8 S	9 7	23 31 13	☽ 16 21 11
22	81	·219	6 13	17 58	0 38 N	7 3	23 58 48	○ 25 1 12
29	88	·238	6 25	17 42	3 23	4 54	0 26 24	
April 5	95	·257	6 37	17 27	6 4	2 47	0 54 0	☾ 1 18 30
12	102	·277	6 48	17 11	8 41	0 0 51	1 21 36	● 8 8 57
19	109	·296	7 0	16 56	11 10	23 59 8	1 49 12	☽ 15 13 40
26	116	·315	7 12	16 42	13 31	57 45	2 16 48	○ 23 17 5
May 3	123	·334	7 23	16 29	15 40	56 47	2 44 24	☾ 1 1 51
10	130	·353	7 34	16 17	17 37	56 16	3 12 0	● 7 18 16
17	137	·372	7 45	16 6	19 20	56 13	3 39 35	☽ 15 7 4
24	144	·392	7 55	15 57	20 46	56 36	4 7 11	○ 23 6 26
31	151	·411	8 3	15 51	21 55	57 25	4 34 47	☾ 30 6 55
June 7	158	·430	8 10	15 46	22 46	58 34	5 2 23	● 6 4 26
14	165	·449	8 15	15 44	23 16	23 59 58	5 29 59	☽ 14 0 34
21	172	·468	8 18	15 45	23 27	0 1 27	5 57 35	○ 21 17 12
28	179	·487	8 19	15 48	23 18	2 56	6 25 11	☾ 28 11 16
July 5	186	·507	8 17	15 53	22 48	4 16	6 52 47	● 5 15 59
12	193	·526	8 12	16 0	22 0	5 21	7 20 23	☽ 13 17 29
19	200	·545	8 5	16 8	20 52	6 2	7 47 59	○ 21 1 54
26	207	·564	7 56	16 18	19 28	6 17	8 15 35	☾ 27 16 33
Aug. 2	214	·583	7 46	16 28	17 47	6 3	8 43 10	● 4 5 12
9	221	·602	7 33	16 39	15 53	5 20	9 10 46	☽ 12 9 12
16	228	·622	7 20	16 50	13 46	4 9	9 38 22	○ 19 9 28
23	235	·641	7 6	17 1	11 28	2 31	10 5 58	☾ 26 0 9
30	242	·660	6 51	17 12	9 1	0 0 33	10 33 34	
Sept. 6	249	·679	6 35	17 23	6 28	23 58 19	11 1 10	● 2 20 16
13	256	·698	6 19	17 34	3 49	55 55	11 28 46	☽ 10 23 7
20	263	·717	6 3	17 46	1 6 N	53 26	11 56 22	○ 17 17 4
27	270	·737	5 47	17 57	1 37 S	51 1	12 23 57	☾ 24 11 7
Oct. 4	277	·756	5 31	18 9	4 21	48 46	12 51 33	● 2 12 58
11	284	·775	5 15	18 20	7 1	46 48	13 19 9	☽ 10 10 57
18	291	·794	5 0	18 32	9 37	45 14	13 46 45	○ 17 1 45
25	298	·813	4 46	18 45	12 6	44 9	14 14 21	☾ 24 1 56
Nov. 1	305	·832	4 32	18 57	14 26	43 40	14 41 57	● 1 6 33
8	312	·852	4 20	19 9	16 35	43 51	15 9 33	☽ 8 20 46
15	319	·871	4 10	19 22	18 30	44 41	15 37 9	○ 15 12 16
22	326	·890	4 1	19 33	20 9	46 13	16 4 45	☾ 22 20 26
29	333	·909	3 54	19 44	21 30	48 24	16 32 20	● 30 23 45
Dec. 6	340	·928	3 50	19 54	22 31	51 9	16 59 56	☽ 8 5 13
13	347	·947	3 49	20 1	23 10	54 18	17 27 32	○ 15 0 53
20	354	·967	3 50	20 6	23 27	23 57 43	17 55 8	☾ 22 17 39
27	361	·986	3 54	20 8	23 20 S	0 1 12	18 22 44	● 30 15 20

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	10 32	17 15	6 26 N	−12·0 E.
2	F	11 40	17 56	1 23 N	+ 0·2 E.
3	S	12 50	18 38	3 49 S	+12·3 E.
4	**S**	14 2	19 22	9 0	+24·5 E.
5	M	15 18	20 9	13 57	+36·6 E.
6	Tu	16 37	21 0	18 24	+48·8 E.
7	W	17 57	21 56	22 2	+61·0 E.
8	Th	19 14	22 57	24 28	+73·1 E.
9	F	sets	......	25 19	+85·3 E.
10	S	3 45	0 1	24 25	......
11	**S**	5 5	1 5	21 45	−70·4 M.
12	M	6 34	2 6	17 37	−58·3 M.
13	Tu	8 2	3 4	12 26	−46·1 M.
14	W	9 29	3 57	6 36	−34·0 M.
15	Th	10 52	4 47	0 34 S	−21·8 M.
16	F	12 13	5 35	5 21 N	− 9·6 M.
17	S	13 32	6 23	10 52	+ 2·5 M.
18	**S**	14 50	7 11	15 45	+14·7 M.
19	M	16 5	8 0	19 48	+26·9 M.
20	Tu	17 17	8 51	22 50	+39·0 M.
21	W	18 21	9 43	24 42	+51·2 M.
22	Th	19 16	10 35	25 20	+63·3 M.
23	F	rises	11 26	24 44	+75·5 M.
24	S	3 48	12 16	22 58	......
25	**S**	4 55	13 4	20 13	−80·2 E.
26	M	6 3	13 49	16 37	−68·0 E.
27	Tu	7 12	14 32	12 23	−55·9 E.
	W	8 21	15 13	7 42	−43·7 E.
		9 28	15 53	2 43 N	−31·6 E.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	12 58	18 0	12 27 S	+ 4·9 E.
2	M	14 14	18 48	16 59	+17·1 E.
3	Tu	15 32	19 40	20 51	+29·3 E.
4	W	16 49	20 37	23 44	+41·4 E.
5	Th	17 59	21 38	25 15	+53·6 E.
6	F	18 57	22 42	25 7	+65·7 E.
7	S	19 42	23 45	23 13	+77·9 E.
8	**S**	sets	......	19 37	......
9	M	5 29	0 46	14 41	−77·8 M.
10	Tu	7 0	1 43	8 51	−65·6 M.
11	W	8 29	2 36	2 35 S	−53·5 M.
12	Th	9 54	3 28	3 40 N	−41·3 M,
13	F	11 16	4 17	9 33	−29·1 M.
14	S	12 37	5 7	14 46	−17·0 M.
15	**S**	13 56	5 56	19 8	− 4·8 M.
16	M	15 9	6 47	22 26	+ 7·4 M.
17	Tu	16 16	7 39	24 34	+19·6 M.
18	W	17 14	8 31	25 27	+31·8 M.
19	Th	18 0	9 23	25 6	+44·0 M.
20	F	18 36	10 13	23 35	+56·1 M.
21	S	rises	11 1	21 2	+68·3 M.
22	**S**	3 54	11 47	17 36	+80·4 M.
23	M	5 2	12 30	13 28	......
24	Tu	6 11	13 12	8 50	−75·2 E.
25	W	7 19	13 52	3 51 N	−63·0 E.
26	Th	8 27	14 33	1 17 S	−50·9 E.
27	F	9 36	15 14	6 26	−38·7 E
28	S	10 47	15 56	11 25 S	−26·5 E.

MARCH.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	12 0	16 42	16 1 S	−14·4 E.
2	M	13 15	17 31	20 2	− 2·2 E.
3	Tu	14 21	18 25	23 9	+10·0 E.
4	W	15 42	19 22	25 6	+22·2 E.
5	Th	16 44	20 23	25 35	+34·3 E.
6	F	17 33	21 25	24 25	+46·5 E.
7	S	18 10	22 25	21 33	+58·7 E.
8	**S**	18 38	23 24	17 11	+70·9 E.
9	M	sets	......	11 39	+83·1 E.
10	Tu	5 53	0 19	5 24 S	......
11	W	7 22	1 13	1 6 N	−72·5 M.
12	Th	8 50	2 4	7 25	−60·4 M.
13	F	10 14	2 56	13 9	−48·2 M.
14	S	11 37	3 47	18 2	−36·0 M.
15	**S**	12 55	4 39	21 49	−23·9 M.
16	M	14 7	5 33	24 21	−11·7 M.
17	Tu	15 11	6 26	25 36	+ 0·5 M.
18	W	16 0	7 18	25 32	+12·7 M.
19	Th	16 39	8 9	24 16	+24·8 M.
20	F	17 9	8 58	21 55	+37·0 M.
21	S	17 31	9 45	18 39	+49·2 M.
22	**S**	17 49	10 28	14 39	+61·4 M.
23	M	rises	11 11	10 5	+73·6 M.
24	Tu	5 9	11 51	5 7 N	......
25	W	6 17	12 32	0 4 S	−82·0 E.
26	Th	7 27	13 13	5 19	−69·8 E.
27	F	8 37	13 55	10 25	−57·6 E.
28	S	9 50	14 40	15 11	−45·4 E.
29	**S**	11 5	15 28	19 22	−33·2 E.

APRIL.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	14 38	18 12	25 52 S	+ 3·3 E.
2	Th	15 30	19 12	25 12	+15·5 E.
3	F	16 9	20 11	22 57	+27·7 E.
4	S	16 39	21 8	19 12	+39·9 E.
5	**S**	17 3	22 4	14 11	+52·0 E.
6	M	17 22	22 57	8 16	+64·2 E.
7	Tu	17 39	23 49	1 50 S	+76·4 E.
8	W	sets	......	4 39 N	......
9	Th	7 42	0 40	10 49	−79·2 M.
10	F	9 9	1 33	16 15	−67·0 M.
11	S	10 33	2 26	20 39	−54·8 M.
12	**S**	11 50	3 20	23 47	−42·6 M.
13	M	13 0	4 15	25 33	−30·4 M.
14	Tu	13 57	5 10	25 55	−18·2 M.
15	W	14 40	6 3	24 59	− 6·0 M.
16	Th	15 13	6 53	22 54	+ 6·2 M.
17	F	15 39	7 41	19 50	+18·4 M.
18	S	15 57	8 26	15 59	+30·6 M.
19	**S**	16 12	9 8	11 32	+42·8 M.
20	M	16 26	9 49	6 38	+55·0 M.
21	Tu	16 39	10 30	1 27 N	+67·2 M.
22	W	rises	11 11	3 52 S	+79·4 M.
23	Th	6 26	11 53	9 6	......
24	F	7 39	12 37	14 4	−76·2 E.
25	S	8 55	13 25	18 30	−64·0 E.
26	**S**	10 11	14 15	22 9	−51·8 E.
27	M	11 25	15 10	24 43	−39·6 E.
28	Tu	12 33	16 7	25 57	−27·4 E.
29	W	13 29	17 6	25 40	−15·2 E.

MAY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	14 43	19 1	20 31 S	+ 9·3 E.
2	S	15 8	19 55	15 58	+21·5 E.
3	**S**	15 27	20 47	10 27	+33·7 E.
4	M	15 45	21 37	4 19 S	+45·9 E.
5	Tu	16 1	22 28	2 4 N	+58·1 E.
6	W	16 18	23 19	8 20	+70·3 E.
7	Th	sets	......	14 5	+82·5 E.
8	F	8 3	0 11	18 59	......
9	S	9 26	1 5	22 44	−73·1 M.
10	**S**	10 42	2 1	25 7	−60·9 M.
11	M	11 47	2 57	26 3	−48·7 M.
12	Tu	12 36	3 52	25 34	−36·5 M.
13	W	13 14	4 45	23 50	−24·2 M.
14	Th	13 41	5 34	21 2	−12·0 M.
15	F	14 3	6 21	17 23	+ 0·2 M.
16	S	14 20	7 4	13 5	+12·4 M.
17	**S**	14 34	7 46	8 18	+24·6 M.
18	M	14 48	8 27	3 11 N	+36·9 M.
19	Tu	15 1	9 7	2 7 S	+49·1 M.
20	W	15 14	9 48	7 25	+61·3 M.
21	Th	15 30	10 32	12 31	+73·5 M.
22	F	15 49	11 18	17 13	......
23	S	rises	12 9	21 12	−82·1 E.
24	**S**	9 12	13 3	24 10	−69·9 E.
25	M	10 25	14 1	25 49	−57·7 E.
26	Tu	11 26	15 0	25 56	−45·4 E.
27	W	12 13	16 0	24 26	−33·2 E.
	Th	12 48	16 57	21 26	−21·0 E.

JUNE.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	14 8	20 21	0 12 N	+27·9 E.
2	Tu	14 24	21 10	6 22	+40·2 E.
3	W	14 42	22 1	12 12	+52·4 E.
4	Th	15 3	22 53	17 20	+64·6 E.
5	F	15 30	23 48	21 29	+76·8 E.
6	S	sets	......	24 23	......
7	**S**	9 31	0 44	25 52	−78·8 M.
8	M	10 28	1 40	25 54	−66·5 M.
9	Tu	11 11	2 34	24 36	−54·3 M.
10	W	11 43	3 26	22 7	−42·1 M.
11	Th	12 7	4 14	18 42	−29·9 M.
12	F	12 26	4 59	14 34	−17·6 M.
13	S	12 40	5 41	9 55	− 5·4 M.
14	**S**	12 54	6 22	4 54 N	+ 6·8 M.
15	M	13 7	7 2	0 19 S	+19·0 M.
16	Tu	13 20	7 43	5 35	+31·2 M.
17	W	13 34	8 25	10 45	+43·5 M.
18	Th	13 52	9 10	15 36	+55·7 M.
19	F	14 15	9 59	19 53	+67·9 M.
20	S	14 45	10 52	23 15	+80·1 M.
21	**S**	rises	11 49	25 24	......
22	M	9 15	12 49	26 2	−75·4 E.
23	Tu	10 9	13 50	25 0	−63·2 E.
24	W	10 49	14 50	22 19	−50·9 E.
25	Th	11 18	15 47	18 15	−38·7 E.
26	F	11 41	16 40	13 6	−26·5 E.
27	S	11 59	17 30	7 17	−14·3 E.
28	**S**	12 15	18 19	1 8 S	− 2·1 E.

JULY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	13 7	20 47	16 5 N	+34·5 E.
2	Th	13 31	21 39	20 26	+46·8 E.
3	F	14 3	22 34	23 39	+59·0 E.
4	S	14 44	23 29	25 32	+71·2 E.
5	**S**	sets	......	26 1	+83·4 E.
6	M	9 6	0 24	25 8	......
7	Tu	9 43	1 17	23 0	−72·2 M.
8	W	10 10	2 7	19 50	−60·0 M.
9	Th	10 30	2 53	15 53	−47·7 M.
10	F	10 46	3 37	11 21	−35·5 M.
11	S	11 0	4 18	6 26	−23·3 M.
12	**S**	11 14	4 58	1 17 N	−11·0 M.
13	M	11 26	5 38	3 56 S	+ 1·2 M.
14	Tu	11 39	6 19	9 5	+13·4 M.
15	W	11 54	7 2	14 0	+25·6 M.
16	Th	12 14	7 48	18 26	+37·9 M.
17	F	12 40	8 38	22 8	+50·1 M.
18	S	13 16	9 33	24 46	+62·3 M.
19	**S**	14 6	10 33	26 0	+74·5 M.
20	M	15 14	11 34	25 35	......
21	Tu	rises	12 36	23 25	−81·0 E.
22	W	9 19	13 36	19 41	−68·8 E.
23	Th	9 44	14 32	14 40	−56·5 E.
24	F	10 4	15 25	8 50	−44·3 E.
25	S	10 21	16 16	2 33 S	−32·1 E.
26	**S**	10 38	17 5	3 45 N	−19·9 E.
27	M	10 54	17 54	9 44	− 7·7 E.
28	Tu	11 12	18 44	15 8	+ 4·5 E.

AUGUST.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	13 29	22 18	26 5 N	+53·4 E.
2	**S**	14 29	23 11	25 32	+65·6 E.
3	M	15 36	......	23 42	+77·8 E.
4	Tu	sets	0 1	20 47	......
5	W	8 35	0 49	17 1	−77·8 M.
6	Th	8 52	1 33	12 36	−65·6 M.
7	F	9 6	2 15	7 45	−53·4 M.
8	S	9 20	2 56	2 39 N	−41·1 M.
9	**S**	9 32	3 35	2 33 S	−28·9 M.
10	M	9 45	4 15	7 43	−16·7 M.
11	Tu	9 59	4 56	12 39	− 4·5 M.
12	W	10 17	5 40	17 11	+ 7·8 M.
13	Th	10 38	6 27	21 5	+20·0 M.
14	F	11 9	7 19	24 5	+32·2 M.
15	S	11 50	8 15	25 51	+44·4 M.
16	**S**	12 49	9 15	26 5	+56·6 M.
17	M	14 4	10 17	24 37	+68·9 M.
18	Tu	15 31	11 18	21 27	+81·1 M.
19	W	rises	12 17	16 48	......
20	Th	8 7	13 13	11 4	−74·5 E.
21	F	8 25	14 6	4 40 S	−62·3 E.
22	S	8 41	14 57	1 55 N	−50·1 E.
23	**S**	8 58	15 48	8 15	−37·9 E.
24	M	9 16	16 39	14 1	−25·7 E.
25	Tu	9 38	17 31	18 54	−13·4 E.
26	W	10 4	18 25	22 39	− 1·2 E.
27	Th	10 39	19 20	25 7	+11·0 E.
28	F	11 24	20 14	26 13	+23·2 E.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	15 47	23 31	18 4 N.	+72·0 E.
2	W	sets	......	13 46	......
3	Th	7 14	0 14	8 59	−83·5 M.
4	F	7 27	0 54	3 53 N.	−71·3 M.
5	S	7 39	1 34	1 21 S.	−59·1 M.
6	**S**	7 52	2 14	6 33	−46·9 M.
7	M	8 5	2 54	11 34	−34·7 M.
8	Tu	8 20	3 36	16 12	−22·5 M.
9	W	8 40	4 22	20 15	−10·3 M.
10	Th	9 6	5 10	23 29	+ 1·9 M.
11	F	9 41	6 3	25 38	+14·1 M.
12	S	10 30	7 0	26 24	+26·3 M.
13	**S**	11 37	7 59	25 37	+38·5 M.
14	M	12 57	8 59	23 9	+50·7 M.
15	Tu	14 26	9 59	19 8	+62·9 M.
16	W	15 58	10 56	13 48	+75·1 M.
17	Th	rises	11 50	7 32	......
18	F	6 45	12 43	0 49 S.	−80·5 E.
19	S	7 1	13 36	5 54 N.	−68·3 E.
20	**S**	7 19	14 28	12 10	−56·1 E.
21	M	7 39	15 22	17 36	−43·9 E.
22	Tu	8 4	16 17	21 53	−31·7 E.
23	W	8 36	17 13	24 49	−19·5 E.
24	Th	9 19	18 9	26 18	− 7·3 E.
25	F	10 13	19 3	26 20	+ 4·9 E.
26	S	11 16	19 55	25 2	+17·1 E.
27	**S**	12 25	20 44	22 34	+29·3 E
28	M	13 37	21 30	19 8	+41·5 E.
29	Tu	14 47	22 13	14 58	+53·7 E.
30	W	15 56	22 54	10 15 N.	+65·9 E.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	17 5	23 34	5 11 N.	+78·1 E.
2	F	sets	......	0 5 S.	......
3	S	6 0	0 13	5 22	−77·5 M.
4	**S**	6 12	0 53	10 30	−65·3 M.
5	M	6 27	1 35	15 17	−53·1 M.
6	Tu	6 45	2 19	19 31	−40·9 M.
7	W	7 7	3 7	22 58	−28·7 M.
8	Th	7 37	3 58	25 24	−16·6 M.
9	F	8 20	4 52	26 33	− 4·4 M.
10	S	9 20	5 49	26 15	+ 7·8 M.
11	**S**	10 32	6 47	24 24	+20·0 M.
12	M	11 55	7 44	21 1	+32·2 M.
13	Tu	13 24	8 40	16 17	+44·4 M.
14	W	14 53	9 34	10 29	+56·5 M.
15	Th	16 24	10 27	3 58 S.	+68·7 M.
16	F	rises.	11 19	2 50 N.	+80·9 M.
17	S	5 21	12 12	9 27	......
18	**S**	5 39	13 6	15 27	−74·8 E.
19	M	6 1	14 2	20 25	−62·6 E.
20	Tu	6 31	15 0	24 3	−50·4 E.
21	W	7 10	15 58	26 8	−38·3 E.
22	Th	8 2	16 55	26 40	−26·1 E.
23	F	9 3	17 49	25 44	−13·9 E.
24	S	10	18 40	23 31	− 1·7 E.
25	**S**	11 24	19 27	20 17	+10·5 E.
26	M	12 35	20 11	16 15	+22·7 E.
27	Tu	13 45	20 53	11 38	+34·9 E.
28	W	14 54	21 33	6 37	+47·0 E.
29	Th	16 2	22 12	1 22 N.	+59·2 E.
30	F	17 11	22 52	3 58 S.	+71·4 E.
31	S	18 21	23 34	9 12 S.	+83·5 E.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	4 34	......	14 9 S.	......
2	M	4 51	0 17	18 36	−72·1 M.
3	Tu	5 11	1 4	22 19	−59·9 M.
4	W	5 40	1 54	25 2	−47·8 M.
5	Th	6 18	2 48	26 31	−35·6 M.
6	F	7 11	3 44	26 35	−23·4 M.
7	S	8 18	4 41	25 8	−11·3 M.
8	**S**	9 37	5 37	22 13	+ 0·9 M.
9	M	11 1	6 32	17 59	+13·1 M.
10	Tu	12 27	7 24	12 40	+25·2 M.
11	W	13 54	8 15	6 35	+37·4 M.
12	Th	15 21	9 6	0 2 S.	+49·5 M.
13	F	16 49	9 57	6 34 N.	+61·7 M.
14	S	18 18	10 49	12 50	+73·9 M.
15	**S**	rises	11 44	18 19	......
16	M	4 26	12 41	22 38	−81·7 E.
17	Tu	5 0	13 41	25 29	−69·6 E.
18	W	5 47	14 40	26 42	−57·4 E.
19	Th	6 45	15 37	26 18	−45·2 E.
20	F	7 54	16 31	24 29	−33·1 E.
21	S	9 7	17 21	21 30	−20·9 E.
22	**S**	10 19	18 7	17 39	− 8·7 E.
23	M	11 31	18 50	13 9	+ 3·4 E.
24	Tu	12 41	19 30	8 12	+15·6 E.
25	W	13 48	20 10	3 0 N.	+27·8 E.
26	Th	14 57	20 49	2 19 S.	+40·0 E.
27	F	16 6	21 30	7 36	+52·1 E.
28	S	17 18	22 13	12 41	+64·3 E.
29	**S**	18 32	22 59	17 20	+76·5 E.
30	M	19 49	23 49	21 20 S.	......

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	3 41	......	24 24 S.	−79·2 M.
2	W	4 17	0 42	26 16	−67·1 M.
3	Th	5 6	1 38	26 41	−54·9 M.
4	F	6 10	2 36	25 34	−42·8 M.
5	S	7 26	3 33	22 58	−30·6 M.
6	**S**	8 48	4 28	19 2	−18·5 M.
7	M	10 12	5 21	14 1	− 6·3 M.
8	Tu	11 38	6 11	8 13	+ 5·8 M.
9	W	13 2	7 0	1 58 S.	+18·0 M.
10	Th	14 25	7 49	4 27 N.	+30·2 M.
11	F	15 50	8 38	10 39	+42·3 M.
12	S	17 17	9 30	16 19	+54·5 M.
13	**S**	18 43	10 25	21 2	+66·7 M.
14	M	rises	11 23	24 28	+78·8 M.
15	Tu	3 34	12 22	26 21	......
16	W	4 26	13 21	26 36	−76·8 E.
17	Th	5 31	14 18	25 19	−64·7 E.
18	F	6 45	15 11	22 42	−52·5 E.
19	S	7 59	15 59	19 4	−40·4 E.
20	**S**	9 13	16 44	14 42	−28·2 E.
21	M	10 24	17 26	9 50	−16·0 E.
22	Tu	11 34	18 6	4 40 N.	− 3·9 E.
23	W	12 42	18 45	0 38 S.	+ 8·3 E.
24	Th	13 50	19 25	5 55	+20·4 E.
25	F	15 0	20 7	11 2	+32·6 E.
26	S	16 13	20 51	15 50	+44·7 E.
27	**S**	17 28	21 39	20 4	+56·9 E.
28	M	18 43	22 31	23 29	+69·1 E.
29	Tu	19 55	23 27	25 47	+81·2 E.
30	W	sets	......	26 41	......
31	Th	3 56	0 26	26 0 S.	−74·5 M.

List of the principal Meteor-showers of the Year, derived from recent Observations. By W. F. Denning.

Date.	Radiant-point.		Notes.	Date.	Radiant-point.		Notes.
	α	δ			α	δ	
Jan. 2 ...	**230°**	**+53°**	Swift; long paths.	July 28 ...	**339°**	**−12°**	Slow; long.
5 ...	140	+57	Swift; short.	30 ...	6	+35	Swift; streaks.
9 ...	221	+42	Swift; streaks.	31 ...	7	+11	Swift; streaks.
17 ...	295	+53	Slow; bright.	Aug. 4 ...	30	+36	Swift; streaks.
22 ...	208	− 8	Very swift; streaks.	10 ...	**45**	**+57**	Swift; streaks.
25 ...	180	+24	Swift.	16 ...	61	+48	Swift; streaks.
Feb. 4 ...	61	+28	Rather swift.	21 ...	73	+41	Swift; streaks.
15 ...	236	+11	Swift; streaks.	22 ...	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Sept. 3 ...	354	+38	Very swift.
20 ...	263	+36	Swift; streaks.	4 ...	346	± 0	Slow; bright.
Mar. 1 ...	47	+45	Very slow.	7 ...	62	+37	Swift; streaks.
14 ...	175	+10	Slow; bright.	15 ...	48	+44	Very swift.
14 ...	280	−14	Swift; streaks.	19 ...	5	+10	Slow; short.
18 ...	316	+76	Slow; bright.	21 ...	31	+19	Slow.
24 ...	161	+58	Swift.	22 ...	335	+58	Slowish.
27 ...	229	+32	Swift; small.	22 ...	63	+22	Swift; streaks.
28 ...	263	+62	Rather swift.	30 ...	25	+71	Small; short.
Apr. 17 ...	165	− 6	Very slow.	Oct. 2 ...	73	+41	Swift; streaks.
18 ...	213	+53	Swift; short.	2 ...	225	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
19 ...	229	− 2	Slow; long.	4 ...	310	+77	Slowish.
20 ...	**270**	**+33**	Swift.	8 ...	42	+55	Slow; trained.
25 ...	272	+21	Swift; short.	8 ...	77	+31	Swift; streaks.
May 1 ...	239	+46	Small; short.	11 ...	13	+ 6	Slow; bright.
5 ...	234	+10	Slowish.	14 ...	40	+20	Rather swift.
5 ...	254	−21	Slowish.	14 ...	135	+68	Swift; streaks.
6 ...	**338**	**− 2**	Swift; streaks.	18 ...	**92**	**+15**	Swift; streaks.
7 ...	244	+ 7	Slow; bright.	20 ...	106	+12	Swift; streaks.
15 ...	295	± 0	Swift; streaks.	29 ...	109	+23	Very swift.
29 ...	264	+64	Slowish.	Nov. 1 ...	43	+22	Slow; bright.
30 ...	333	+27	Swift; streaks.	2 ...	55	+ 9	Slow; bright.
June 7 ...	247	−25	Slow; very bright.	13 ...	**150**	**+ 2**	Swift; streaks.
13 ...	310	+61	Swift; streaks.	16 ...	154	+41	Swift; streaks.
15 ...	291	+52	Swift; small.	17 ...	53	+71	Slowish.
15 ...	285	+23	Slowish.	20 ...	62	+23	Slow; bright.
18 ...	302	+24	Swift.	27 ...	**25**	**+44**	Very slow; trains.
20 ...	335	+57	Swift.	30 ...	190	+58	Swift; streaks.
28 ...	294	+39	Slow.	Dec. 4 ...	162	+58	Swift; streaks.
July 4 ...	303	+24	Swift.	4 ...	110	+25	Slowish.
5 ...	21	+23	Swift; streaks.	5 ...	105	+11	Slow.
11 ...	349	+53	Swift.	6 ...	80	+23	Slow; bright.
19 ...	314	+48	Swift; short.	8 ...	145	+ 7	Swift; streaks.
20 ...	269	+49	Swift.	8 ...	208	+71	Rather swift.
22 ...	16	+31	Swift; streaks.	10 ...	**108**	**+33**	Swift; short.
23 ...	335	+49	Swift; short.	18 ...	134	+ 8	Very swift.
25 ...	48	+43	Swift; streaks.	22 ...	194	+67	Swift; streaks.

The radiant-point of the most brilliant showers are indicated by heavier type.

Some of the above systems remain visible for a considerable period; thus the Perseids, with maximum on August 10, form a rich display from July 25 to August 15, and the radiant-point advances from about 27° +54° to 56° +57°. On August 1 it is placed at 33° +56°, and on August 5 at 38° +57°.

							Venus (♀).						Mars (♂).					
Date.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	20 7 18	20 39 S.	7·6	21 9	1 24	5 35	16 18 0	16 49 S.	47·0	16 58	21 32	2 10	22 53 14	7 59 S.	6·2	22 44	4 9	9 33
11...	19 50 34	18 26	9·8	19 52	0 28	4 53	16 36 39	17 11	39·8	16 40	21 12	1 47	23 20 41	4 56	6·0	22 16	3 57	9 37
21...	19 4 51	19 17	9·4	18 38	22 57	3 23	17 4 57	18 8	34·0	16 36	21 2	1 30	23 47 49	1 50 S.	5·8	21 48	3 45	9 41
31...	19 11 25	20 43	7·6	18 18	22 29	2 41	17 40 9	19 8	29·6	16 37	20 58	1 21	0 14 43	1 15 N.	5·6	21 21	3 33	9 44
Feb. 10...	19 52 46	20 56	6·4	18 22	22 33	2 42	18 20 18	19 47	26·2	16 43	20 59	1 15	0 41 32	4 18	5·4	20 53	3 20	9 46
20...	20 48 2	19 13	5·6	18 27	22 49	3 7	19 3 43	19 49	23·4	16 47	21 3	1 19	1 8 22	7 15	5·2	20 26	3 7	9 49
Mar. 2...	21 49 13	15 22	5·2	18 26	23 12	3 51	19 49 6	19 4	21·2	16 48	21 9	1 30	1 35 20	10 4	5·0	19 58	2 55	9 51
12...	22 53 55	9 21	5·0	18 19	23 37	4 51	20 35 21	17 28	19·4	16 44	21 17	1 47	2 2 31	12 43	4·8	19 33	2 43	9 53
22...	0 2 28	1 19 S.	5·0	18 6	0 4	6 2	21 21 31	15 3	17·8	16 37	21 23	2 8	2 30 0	15 10	4·7	19 5	2 31	9 55
Apr. 1...	1 15 3	8 2 N.	5·2	17 52	0 37	7 20	22 7 9	11 54	16·6	16 26	21 29	2 31	2 57 49	17 22	4·6	18 39	2 19	9 57
11...	2 23 24	16 18	6·4	17 36	1 6	8 37	22 52 2	8 10	15·6	16 11	21 35	2 55	3 26 0	19 19	4·5	18 16	2 8	9 58
21...	3 9 20	20 40	8·4	17 13	1 12	9 10	23 36 16	4 0 S.	14·6	15 55	21 40	3 21	3 54 31	20 58	4·4	17 55	1 57	9 57
May 1...	3 20 38	20 28	10·8	16 44	0 44	8 40	0 20 11	0 25 N.	13·8	15 36	21 44	3 50	4 23 21	22 19	4·2	17 38	1 47	9 55
11...	3 4 10	16 42	12·0	16 12	23 42	7 21	1 4 16	4 55	13·2	15 18	21 49	4 16	4 52 24	23 20	4·1	17 19	1 36	9 52
21...	2 50 47	13 14	11·2	15 40	22 52	6 9	1 49 1	9 18	12·6	15 1	21 54	4 45	5 21 34	24 0	4·0	17 4	1 26	9 47
31...	3 1 53	13 14	9·2	15 13	22 26	5 39	2 34 57	13 24	12·0	14 47	22 1	5 12	5 50 42	24 20	4·0	16 52	1 16	9 39
June 10...	3 38 8	16 17	7·4	14 54	22 25	5 53	3 22 31	17 1	11·6	14 34	22 9	5 43	6 19 42	24 20	4·0	16 40	1 5	9 29
20...	4 37 51	20 39	6·0	14 49	22 47	6 40	4 11 53	19 57	11·2	14 26	22 19	6 10	6 48 25	23 59	3·9	16 33	0 54	9 16
30..	6 0 59	23 54	5·2	15 12	23 33	7 48	5 3 0	22 1	11·0	14 24	22 31	6 36	7 16 44	23 20	3·8	16 26	0 43	8 59
July 10...	7 35 8	23 21	5·0	16 14	0 23	8 38	5 55 27	23 5	10·6	14 31	22 44	6 56	7 44 34	22 22	3·8	16 24	0 32	8 40
20..	8 58 28	18 56	5·2	17 26	1 7	8 52	6 48 30	23 3	10·4	14 45	22 58	7 11	8 11 50	21 8	3·8	16 20	0 20	8 20
30...	10 4 31	12 41	5·8	18 28	1 33	8 42	7 41 16	21 53	10·3	15 7	23 11	7 16	8 38 29	19 38	3·8	16 16	0 7	7 58
Aug. 9...	10 55 32	6 10	6·6	19 13	1 45	8 19	8 33 0	19 40	10·2	15 33	23 23	7 13	9 4 32	17 55	3·8	16 14	23 52	7 33
19...	11 31 57	0 28 N.	7·6	19 36	1 42	7 47	9 23 10	16 30	10·1	16 4	23 34	7 5	9 30 0	16 1	3·8	16 11	23 38	7 8
29...	11 48 21	3 0 S.	9·0	19 27	1 18	7 7	10 11 39	12 34	10·0	16 35	23 43	6 52	9 54 55	13 56	3·8	16 8	23 24	6 43
Sept. 8...	11 34 4	1 44 S.	10·4	18 21	0 25	6 21	10 58 40	8 4	10·0	17 7	23 50	6 36	10 19 21	11 42	3·8	16 5	23 8	6 14
18...	11 3 57	4 7 N.	9·6	16 44	23 10	5 40	11 44 39	3 11 N.	10·0	17 39	23 57	6 16	10 43 21	9 22	3·9	16 3	22 53	5 46
28...	11 14 41	6 3	7·0	16 14	22 47	5 22	12 30 14	1 52 S.	10·0	18 10	0 2	5 57	11 7 2	6 56	4·0	16 0	22 37	5 17
Oct. 8...	12 6 58	1 22 N.	5·6	16 53	23 2	5 12	13 16 5	6 54	10·0	18 42	0 9	5 37	11 30 29	4 26	4·0	15 57	22 21	4 48
18...	13 9 24	5 48 S.	4·8	17 54	23 25	4 58	14 2 54	11 41	10·1	19 15	0 16	5 19	11 53 47	1 55 N.	4·1	15 54	22 5	4 20
28...	14 11 33	12 48	4·6	18 54	23 48	4 43	14 51 15	16 0	10·2	19 48	0 25	5 5	12 17 1	0 38 S.	4·2	15 50	21 49	3 52
Nov. 7...	15 13 43	18 41	4·6	19 48	0 8	4 31	15 41 34	19 39	10·3	20 21	0 36	4 53	12 40 18	3 9	4·2	15 46	21 33	3 22
17...	16 17 15	23 2	4·8	20 41	0 32	4 28	16 33 54	22 23	10·4	20 51	0 49	4 48	13 3 42	5 38	4·3	15 42	21 17	2 54
27..	17 21 59	25 28	5·2	21 23	0 58	4 35	17 27 53	24 3	10·6	21 17	1 3	4 52	13 27 18	8 4	4·4	15 39	21 1	2 25
Dec. 7...	18 23 5	25 37	6·2	21 43	1 19	4 56	18 22 43	24 30	10·8	21 35	1 19	5 4	13 51 11	10 23	4·6	15 36	20 46	1 58
17...	19 1 58	23 37	7·8	21 23	1 18	5 11	19 17 21	23 43	11·1	21 44	1 34	5 25	14 15 24	12 36	4·8	15 33	20 31	1 31
27..	18 37 55	21 4 S.	9·8	19 55	0 15	4 24	20 10 43	21 43	11·4	21 44	1 48	5 52	14 40 0	14 40 S.	5·0	15 30	20 16	1 4

Mercury.

d h d h d h
Gr. elong. E. ... Apr. 18 7; Aug. 16 11; Dec. 1 3
,, W. ... Feb. 5 22; June 5 9; Sept. 28 9
Sup. conj. ...... Mar. 23 13; July 6 18; Oct. 27 15 d h
Inf. conj. .... Jan. 13 6; May 9 15; Sept. 12 17; Dec. 28 10
Stationary ...... Jan. 3 21; Jan. 24 12; Apr. 29 2; May 21 21
,, ... Aug. 29 20; Sept. 21 3; Dec. 18 23

Venus.

d h
Gr. elong. W. .... Feb. 13 9
Sup. conj. .... Sept. 18 4
Greatest brilliancy ... Jan. 8 5

Mars.

d h
Conjunction ...... July 29 20

Jupiter (♃). Saturn (♄).

1891.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1	21 9 11	17 11 S.	32·3	21 48	2 25	6 59	11 15 52	6 53 N.	17·6	9 51	16 30	23 9
21	21 27 16	15 49	31·5	20 41	1 25	6 6	11 14 2	7 10	18·1	8 29	15 9	21 49
Feb. 10	21 45 57	14 18	31·4	19 33	0 25	5 14	11 9 53	7 41	18·6	7 4	13 46	20 28
Mar. 2	22 4 31	12 41	31·5	18 24	23 22	4 23	11 4 17	8 19	18·6	5 36	12 22	19 8
22	22 22 19	11 4	32·0	17 14	22 21	3 31	10 58 30	8 55	18·5	4 9	10 58	17 47
Apr. 11	22 38 46	9 31	33·1	16 3	21 18	2 36	10 53 44	9 23	18·3	2 42	9 34	16 26
May 1	22 53 12	8 8	34·6	14 51	20 14	1 41	10 51 0	9 37	17·8	1 20	8 13	15 6
21	23 4 59	7 0	36·5	13 39	19 7	0 39	10 50 45	9 35	17·1	0 1	6 54	13 47
June 10	23 13 20	6 14	38·8	12 26	17 56	23 26	10 53 4	9 18	16·4	22 43	5 38	12 29
30	23 17 32	5 53	41·4	11 8	16 42	22 16	10 57 41	8 46	15·9	21 32	4 24	11 12
July 20	23 17 2	6 3	44·0	9 50	15 22	20 54	11 4 13	8 3	15·5	20 24	3 12	9 56
Aug. 9	23 11 55	6 41	46·1	8 29	13 59	19 29	11 12 8	7 12	15·2	19 17	2 1	8 41
29	23 3 21	7 39	47·4	7 7	12 31	17 55	11 20 58	6 16	15·0	18 12	0 51	7 26
Sept. 18	22 53 28	8 40	47·0	5 43	11 3	16 23	11 30 11	5 18	15·0	17 8	23 38	6 12
Oct. 8	22 45 35	9 27	45·6	4 19	9 36	14 53	11 39 17	4 21	15·1	16 2	22 29	4 58
28	22 41 30	9 47	43·3	2 59	8 14	13 29	11 47 42	3 30	15·3	14 57	21 18	3 43
Nov. 17	22 42 25	9 37	40·6	1 41	6 56	12 11	11 54 55	2 47	15·8	13 49	20 7	2 29
Dec. 7	22 48 13	8 58	38·1	0 24	5 43	11 2	12 0 21	2 17	16·3	12 38	18 53	1 12
27	22 58 11	7 53 S.	35·8	23 6	4 35	10 0	12 3 31	2 2 N.	16·8	11 24	17 38	23 52

Uranus (♅). Neptune (♆).

Jan. 1	13 55 44	11 17 S.	3·6	14 4	19 9	0 18	4 11 23	19 25 N.	2·6	1 36	9 26	17 16
31	13 57 46	11 27	3·7	12 8	17 13	22 18	4 9 30	19 22	2·6	23 33	7 27	15 17
Mar. 2	13 56 40	11 20	3·7	10 9	15 14	20 19	4 9 38	19 24	2·6	21 35	5 29	13 19
Apr. 1	13 53 0	11 0	3·8	8 5	13 12	18 19	4 11 50	19 32	2·6	19 39	3 33	11 23
May 1	13 48 12	10 33	3·8	6 0	11 10	16 20	4 15 37	19 42	2·5	17 44	1 39	9 30
31	13 44 6	10 11	3·7	3 55	9 7	14 19	4 20 12	19 54	2·5	15 50	23 42	7 38
June 30	13 42 10	10 1	3·6	1 55	7 8	12 21	4 24 43	20 5	2·5	13 55	21 48	5 45
July 30	13 43 7	10 8	3·6	23 55	5 11	10 23	4 28 19	20 12	2·6	12 0	19 54	3 52
Aug. 29	13 46 51	10 30	3·5	22 3	3 17	8 27	4 30 16	20 15	2·6	10 3	17 58	1 57
Sept. 28	13 52 45	11 3	3·5	20 14	1 25	6 32	4 30 11	20 13	2·6	8 6	16 0	23 54
Oct. 28	13 59 47	11 42	3·4	18 26	23 30	4 38	4 28 10	20 7	2·7	6 7	14 0	21 53
Nov. 27	14 6 45	12 19	3·4	16 39	21 39	2 43	4 24 53	20 0	2·7	4 5	11 58	19 51
Dec. 27	14 12 19	12 48 S.	3·5	14 49	19 46	0 47	4 21 28	19 52 N.	2·7	2 5	9 57	17 49

	Conjunction.	Opposition.	Quadrature.	Stationary.
	d h	d h	d h d h	d h d h
Jupiter	Feb. 13 2	Sept. 5 11	June 7 5; Dec. 1 10	July 7 22; Nov. 3 11
Saturn	Sept. 13 1	Mar. 4 3	May 31 23; Dec. 21 11	May 12 21
Uranus	Oct. 24 23	Apr. 19 6	Jan. 21 3; July 19 22	Feb. 4 7; July 5 13
Neptune	May 27 16	Nov. 29 15	Feb. 22 10; Sept. 1 8	Feb. 13 2; Sept. 11 20

Ceres. Pallas.

1891.	R.A.	Dec.	Transits.	1891.	R.A.	Dec.	Transits.
	h m	° ′	h m		h m	° ′	h m
June 23 ...	22 35·7	20 40 S.	16 28	May 30 ..	20 19·6	18 2 N.	15 46
July 17 ...	22 34·4	22 50	14 52	June 23 ...	20 10·4	19 33	14 2
Aug. 10 ...	22 20·4	25 39	13 3	July 17 ...	19 52·9	18 58	12 10
Sept. 3 ...	22 0·1	27 44	11 8	Aug. 10 ...	19 34·8	15 58	10 18
27 ...	21 45·2	28 6	9 19	Sept. 3 ...	19 24·0	11 27	8 33
Oct. 21 ...	21 45·3	26 53 S.	7 43	27 ...	19 24·6	6 44 N.	6 59

Opposition:—*Ceres*, August 18; *Pallas*, July 25.

Juno.				**Vesta.**			
1891.	R.A.	Dec.	Transits.	1891.	R.A.	Dec.	Transits.
	h m	° ′	h m		h m	° ′	h m
June 23 ...	21 59·7	0 39 S.	15 51	Apr. 24 ...	18 30·2	17 58 S.	16 19
July 17 ...	21 57·7	0 48	14 15	May 18 ...	18 34·6	18 28	14 48
Aug. 10 ...	21 43·4	2 59	12 26	June 11 ...	18 20·3	19 48	12 59
Sept. 3 ...	21 24·5	6 47	10 33	July 5 ...	17 56·6	21 38	11 1
27 ...	21 14·2	10 31	8 48	29 ...	17 41·6	23 20	9 12
Oct. 21 ...	21 19·8	12 50 S.	7 20	Aug. 22 ...	17 45·5	24 43 S.	7 42

Opposition:—*Juno*, Aug. 17; *Vesta*, June 23.

ECLIPSES IN 1891.

I.—*A Total Eclipse of the Moon, May* 23.

Partial Eclipse at Greenwich. At Mid-Eclipse the Moon is in the zenith of 82° E., 20½° S. The first Contact with the shadow occurs at 124° to E. of N. point; last at 90° to W. (for *Direct* image). At Greenwich the Moon rises at $7^h\ 56^m$.

	d h m		h m
First Contact with Penumbra ...	May 23 3 36·9	Shadow	4 41·3
Second " " "	" 9 21·5	"	8 17·1

Total from $5^h\ 49^m{\cdot}7$ to $7^h\ 8^m{\cdot}7$.

Magnitude of Eclipse (Moon's diameter = 1) 1·299.

II.—*An Annular Eclipse of the Sun, June* 6.

Central only in the extreme N.E. of Asia. Partial Eclipse for N. America and N.W. Europe. In the British Isles the Eclipse begins about 5^h in the afternoon and lasts till after 6^h.

Greenwich M.T. of ☌ in R.A. June $6^d\ 4^h\ 38^m\ 0^s{\cdot}2$.

☉'s and ☾'s R.A. $4^h\ 57^m\ 37^s{\cdot}46$. Hourly motions $10^s{\cdot}31$ and $143^s{\cdot}96$.

	° ′ ″		′ ″
☉'s Declination............	22 40 47·6 N.	Hourly motion	0 15·4
☾'s Declination............	22 37 58·0	Hourly motion	6 36·3
☉'s Equat. Hor. Parallax	0 8·7	☉'s true Semidiameter.	15 47·5
☾'s Equat. Hor. Parallax	57 32·0	☾'s true Semidiameter.	15 42·2

	d h m		° ′		° ′
Eclipse begins......	June 6 2 3·4	in Long.	132 33 W.,	Lat.	25 15 N.
Eclipse ends	6 28·0		17 54 E.,		45 50 N.

III.—*A Total Eclipse of the Moon, November* 15.

Visible at Greenwich. The first Contact with the Shadow is at 55° to E. of N.; the last at 95° to W. (for *Direct* image).

	d h m		h m
First Contact with Penumbra ...	Nov. 15 9 36·7	Shadow	10 35·1
Last " " "	" " 15 1·1	"	14 2·7

Total from $11^h\ 37^m{\cdot}4$ to $13^h\ 0^m{\cdot}4$.

Magnitude of Eclipse (Moon's diameter = 1) 1·386.

IV.—*A Partial Eclipse of the Sun, Nov.* 30–*Dec.* 1.

Invisible at Greenwich. Visible only in Antarctic Ocean.

Transit of Mercury.

There will be a Transit of Mercury on May 9, only just visible at Greenwich, Egress occurring 30^m after Sunrise.

Ingress.	d h m s	Egress.	h m s
External Contact, May	9 11 53 44;	Internal Contact,	16 45 31
Internal " "	11 58 41;	External "	16 50 25

Sun in Zenith:—Ingress 179° E.; 18° N. Egress 107° E.; 18° N.

OCCULTATIONS, 1891. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
			h m	°	h m	°
Jan.						
4	2 Libræ ...	6	17 28	164	18 25	262
6	ω^1 Scorpii..	4	17 20	161	18 6	249
13	τ^1 Aquarii .	6	6 29	106	7 11	196
13	τ^2 Aquarii .	4	7 28	63	8 24‡	243
17	ξ^1 Ceti	4½	3 29	81	4 33	215
17	ξ Arietis ...	5½	11 5	57	12 6	259
17	B.A.C. 755 .	6	12 2	108	12 47	211
19	ω^1 Tauri ...	6	11 44	59	12 48	275
19	NEPTUNE ...	...	15 13	128	15 50	213
22	B.A.C. 2154	6½	3 46	64	4 41	279
22	ω Geminor.	6	17 25	173	17 39	202
26	42 Leonis ...	6	15 14	96	16 24	322
28	ν Virginis .	4½	10 56	118	12 7	296
31	96 Virginis .	6½	18 56	99	20 8	319
Feb.						
1	μ Libræ ...	6	13 59	105	15 5	315
12	29 Ceti	6½	6 8	84	7 7	215
12	33 Ceti	6	7 37	71	8 36	234
12	35 Ceti	6½	8 42	125	9 10	184
17	121 Tauri .	6	7 2	47	8 12	290
Mar.						
2	B.A.C. 5395	6	13 5‡	127	14 9	281
13	31 Arietis...	6	8 58	22	9 34	299
18	52 Geminor.	6	9 56	105	11 9	275
22	B.A.C. 3579	6	5 56	146	6 52	253
22	ι Leonis ...	6	7 54	119	9 14	292
26	ρ^2 Virginis .	5	10 1	105	11 11	322
28	ν^1 Libræ ..	6	11 9	95	12 13	326
28	ν^2 Libræ ...	5½	11 28	135	12 37	285
Apr.						
14	37 Geminor.	6	9 16	68	10 12	309
18	42 Leonis...	6	10 35	131	11 46	288
18	B.A.C. 3579	6	14 43	73	15 25	335
20	ν Virginis...	4½	6 7	106	7 19	313
25	41 Libræ ...	6	8 51‡	149	9 45	265
25	κ Libræ ...	5	10 36	153	11 35	263
May						
10	121 Tauri..	6	9 11	142	9 42	215
28	35 Capricor.	6	15 30	61	16 44	259
June						
19	λ Libræ ...	6	7 50	35	8 1	19
19	B.A.C. 5281	6	10 23	127	11 39	278
30	ξ Arietis ..	5½	12 6‡	82	12 55	230
July						
2	ω^2 Tauri ...	5½	14 39	36	15 23	
22	B.A.C. 7550	6	13 17	355	13 40	
23	τ^1 Aquarii .	6	16 3	87	17 2	
Aug.						
1	37 Geminor.	6	15 35	64	16 28	
9	46 Virginis	6	9 14	119	10 13	
14	26 Ophiuchi	6	8 30	139	9 32	
21	30 Piscium .	5	8 6‡	110	8 45	
Sept.						
11	B.A.C. 5909	6½	5 19	114	6 38	
16	τ^1 Aquarii .	6	12 39	106	13 23	
16	τ^2 Aquarii .	4	13 42	69	14 43	
22	53 Tauri ...	6	10 9	93	11 0	
26	λ Cancri ...	6	16 56	21	17 14	
Oct.						
6	B.A.C. 5109	6	6 27	75	7 25	
15	30 Piscium .	5	5 50	114	6 27	
20	99 Tauri ...	6	11 56	8	12 28	
20	103 Tauri...	6	16 54	92	18 6	
22	39 Geminor.	6½	12 9	60	13 10	
22	40 Geminor.	6½	12 24	100	13 29	
26	ι Leonis ...	6	15 48	193	15 57	
Nov.						
7	ω Sagittarii	5	5 21	84	6 37	
7	A Sagittarii	5	7 6	102	8 7	
8	B.A.C. 7237	6	4 34	89	5 48	
10	τ^1 Aquarii .	6	6 31	28	7 33	
16	κ^2 Tauri ...	6½	9 33	25	10 21	
16	τ Tauri......	4½	17 46	106	18 39	
17	118 Tauri...	6	10 37	39	11 33	
22	η Leonis ...	3½	9 51‡	81	10 41	
24	ν Virginis .	4½	14 37	48	15 1	
28	5 Libræ ...	6	18 28	165	19 17	
Dec.						
9	B.A.C. 81...	6½	10 46	17	11 32	
13	56 Tauri ...	6	18 47†	41	19 24	
14	99 Tauri ...	6	8 32	27	9 22	
14	103 Tauri...	6	13 40	98	14 47	
16	39 Geminor.	6½	7 42	47	8 29	
16	40 Geminor.	6½	7 50	88	8 51	
23	46 Virginis.	6	17 52	122	19 13	

† Star setting. ‡ Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i.e.* from th bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	Oct.			h m	°
30	65 Virginis...	6	18 59	31	21	139 Tauri ...	5½	12 4	347
June					Nov.				
16	l^2 Virginis ...	5	10 47	30	4	B.A.C. 5709.	6	4 51	8
18	ν^2 Libræ......	5½	11 22	25	4	26 Ophiuchi.	6	4 56	8
July					10	τ^2 Aquarii ...	4	8 25	328
16	41 Libræ......	6	12 16	15	16	56 Tauri......	6	7 21	338
26	*f* Piscium ...	5½	11 2	334	16	κ^1 Tauri	4½	10 0	337
Sept.					20	λ Cancri	6	8 13	4
15	B.A.C. 7550.	6	10 29	335	Dec.				
22	κ^2 Tauri......	6½	13 44	337	15	139 Tauri ...	5½	8 20	347
23	118 Tauri ...	6	16 36	347	17	λ Cancri......	6	19 47	18

South Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	Oct.			h m	°
6	ω^2 Scorpii ..	4½	18 8	205	19	B.A.C. 1242.	6	13 36	156
24	γ Cancri ...	4½	15 27	200	26	B.A.C. 3579.	6	13 56	197
Feb.					Nov.				
18	B.A.C. 2154.	6½	11 53	185	16	53 Tauri......	6	6 55	158
May					22	42 Leonis ...	6	19 32	211
22	28 Libræ ...	6	8 32	209	Dec.				
					13	B.A.C. 1242.	6	10 29	157
					24	*m* Virginis...	6	17 14	215

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	April. 16h 30m	May. 15h	June. 14h	July. 13h 30m	Aug. 12h 30m	Sept. 11h	Oct. 9h 30m	Nov. 8h	Dec. 6h 30m	Day.
1	32O14	O143●	421O3	431O2	234O1	12O34	1O34●	32O14	43O2●	1
2	21O34	1O234	432O1	43O12	41O32	2O134	3O124	31O4●	41O23	2
3	O1234	2O134	31O2●	4321O	4O123	134O2	312O4	O3124	24O13	3
4	O243●	♃21O4	♃3O14	42O1●	421O3	34O12	32O14	12O34	12O43	4
5	21O43	3O124	2O4●●	41O23	42O31	4321O	1O24●	2O134	3O124	5
6	342O1	3O124	♃O234	♃O13●	43O2●	432O1	O4123	1O324	312O4	6
7	431O2	231O4	O1234	2O143	♃431O	4O132	421O3	3O124	32O14	7
8	♃43O1	2O134	21O34	31O24	234O1	412O3	421O3	32O4●	31O24	8
9	421O3	14O23	23O14	3O124	1O432	42O13	43O12	3412O	♃O234	9
10	4O123	♃4O13	31O24	321O4	O1243	413O2	4312O	4O12●	2O134	10
11	4O23●	421O3	3O241	23O14	21O34	34O12	432O1	♃41O3	12O43	11
12	♃42O3	43O12	2341O	1O234	2O314	321O4	413O2	42O13	O4312	12
13	342O1	43O2●	♃4O23	♃O134	31O24	32O14	4O123	41O32	♃341O	13
14	31O42	4321O	4O123	2O43●	♃♃3O4	O324●	241O3	43O12	432O1	14
15	♃3O14	42O31	421O3	♃31O2	32O14	♃1O34	♃2O43	432O●	431O2	15
16	21O4●	41O23	♃42O1	34O12	1O324	2O134	3O124	3421O	4O132	16
17	O2134	4O213	431O2	4321O	O4123	♃1O24	♃31O4	O412●	42O3●	17
18	1O234	21O34	43O21	423O1	421O3	3O124	32O14	♃1O43	421O3	18
19	2O134	3O214	2431O	41O23	42O31	32 O4	13O24	2O134	4O312	19
20	23O14	3O24●	O143●	4O213	431O2	342O1	O1234	1O234	314O2	20
21	31O24	♃32O4	O243●	421O3	43O1	41O32	21O34	3O124	32O1●	21
22	♃3O21	2O314	21O34	♃43O2	432O●	♃4O23	2O143	321O4	31O4●	22
23	421O●	1O234	2O314	34O12	41O2●	42O13	43O2●	♃32O4	O3124	23
24	4O13●	O2134	31O24	321O4	4O123	41O23	341O2	O124	21O34	24
25	41O23	21O43	3O214	23O14	124O3	43O12	432O1	1O423	21O34	25
26	42O13	34O1●	321O4	1O234	2O413	4321O	413O●	24O13	O1324	26
27	423O●	341O2	O314●	O2134	31O24	342O1	4O132	41O3●	31O24	27
28	431O2	4321O	O423●	21O34	3O124	1O32●	421O3	43O12	32O14	28
29	34O12	423O1	♃42O3	♃O14●	32O4●	♃O243	42O13	4321O	♃31O●	29
30	213O●	41O23	42O13	3O124	31O4●	2O134	♃41O2	432O1	4O12●	30
31		4O213		312O4	O1234		31O2●		421O3	31

(O) *represents Jupiter* ; ♃ signifies that the satellite is on the disk; *signifies that the satellite is* behind the disk or in the shadow.

JUPITER'S SATELLITES, 1891.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

April.

d	h	m	
1	1	37	i. *e*.
	4	43	i. O.
	10	54	ii. *t*.
	13	50	ii. T.
	23	44	i. *t*.
2	1	45	iii. *e*.
	2	4	i. T.
	8	36	iii. O.
	20	6	i. *e*.
	23	13	i. O.
3	4	25	ii. *e*.
	9	0	ii. O.
	18	15	i. *t*.
	20	34	i. T.
4	14	34	i. *e*.
	17	43	i. O.
5	0	18	ii. *t*.
	3	14	ii. T.
	12	45	i. *t*.
	15	4	i. T.
	18	8	iv. *t*.
	19	17	iii. *t*.
	22	51	iv. T.
	22	55	iii. T.
6	9	3	i. *e*.
	12	13	i. O.
	17	44	ii. *e*.
	22	24	ii. O.
7	7	15	i. *t*.
	9	34	i. T.
8	3	31	i. *e*.
	6	43	i. O.
	13	41	ii. *t*.
	16	37	ii. T.
9	1	45	i. *t*.
	4	4	i. T.
	5	46	iii. *e*.
	9	15	iii. E.
	9	24	iii. *o*.
	13	1	iii. O.
	22	0	i. *e*.
0	1	13	i. O.
	7	2	ii. *e*.
	11	48	ii. O.
	20	15	i. *t*.
	22	35	i. T.
1	16	28	i. *e*.
	19	43	i. O.
2	3	5	ii. *t*.
	6	1	ii. T.
	14	45	i. *t*.
	17	4	i. T.

April (con.).

d	h	m	
12	23	42	iii. *t*.
13	3	19	iii. T.
	10	57	i. *e*.
	14	13	i. O.
	17	52	iv. *e*.
	20	20	ii. *e*.
	22	24	iv. E.
14	1	12	ii. O.
	3	1	iv. *o*.
	7	39	iv. O.
	9	15	i. *t*.
	11	34	i. T.
15	5	25	i. *e*.
	8	43	i. O.
	16	29	ii. *t*.
	19	24	ii. T.
16	3	45	i. *t*.
	6	4	i. T.
	9	46	iii. *e*.
	13	15	iii. E.
	13	49	iii. *o*.
	17	25	iii. O.
	23	54	i. *e*.
17	3	13	i. O.
	9	38	ii. *e*.
	14	35	ii. O.
	22	15	i. *t*.
18	0	34	i. T.
	18	22	i. *e*.
	21	43	i. O.
19	5	52	ii. *t*.
	8	47	ii. T.
	16	44	i. *t*.
	19	4	i. T.
20	4	6	iii. *t*.
	7	41	iii. T.
	12	51	i. *e*.
	16	12	i. O.
	22	56	ii. *e*.
21	3	58	ii. O.
	11	14	i. *t*.
	13	34	i. T.
22	7	19	i. *e*.
	10	42	i. O.
	14	29	iv. *t*.
	19	3	iv. T.
	19	15	ii. *t*.
	22	10	ii. T.
23	5	44	i. *t*.
	8	4	i. T.
	13	46	iii. *e*.
	17	15	iii. E.

April (con.).

d	h	m	
23	18	11	iii. *o*.
	21	45	iii. O.
24	1	48	i. *e*.
	5	12	i. O.
	12	14	ii. *e*.
	17	21	ii. O.
25	0	14	i. *t*.
	2	33	i. T.
	20	16	i. *e*.
	23	42	i. O.
26	8	37	ii. *t*.
	11	32	ii. T.
	18	43	i. *t*.
	21	3	i. T.
27	8	27	iii. *t*.
	12	1	iii. T.
	14	45	i. *e*.
	18	11	i. O.
28	1	32	ii. *e*.
	6	43	ii. O.
	13	13	i. *t*.
	15	32	i. T.
29	9	13	i. *e*.
	12	41	i. O.
	22	0	ii. *t*.
30	0	55	ii. T.
	7	43	i. *t*.
	10	2	i. T.
	12	3	iv. *e*.
	16	32	iv. E.
	17	46	iii. *e*.
	21	15	iii. E.
	22	31	iii. *o*.
	23	7	iv. *o*.

May.

d	h	m	
1	2	4	iii. O.
	3	35	iv. O.
	3	42	i. *e*.
	7	10	i. O.
	14	49	ii. *e*.
	20	5	ii. O.
2	2	12	i. *t*.
	4	32	i. T.
	22	10	i. *e*.
3	1	40	i. O.
	11	22	ii. *t*.
	14	17	ii. T.
	20	42	i. *t*.
	23	1	i. T.

May (con.).

d	h	m	
4	12	46	iii. *t*.
	16	19	iii. T.
	16	39	i. *e*.
	20	9	i. O.
5	4	7	ii. *e*.
	9	27	ii. O.
	15	11	i. *t*.
	17	31	i. T.
6	11	7	i. *e*.
	14	39	i. O.
7	0	44	ii. *t*.
	3	38	ii. T.
	9	41	i. *t*.
	12	0	i. T.
	21	46	iii. *e*.
8	1	14	iii. E.
	2	47	iii. *o*.
	5	36	i. *e*.
	6	19	iii. O.
	9	8	i. O.
	17	25	ii. *e*.
	22	48	ii. O.
9	4	10	i. *t*.
	6	29	i. T.
	10	20	iv. *t*.
	14	43	iv. T.
10	0	4	i. *e*.
	3	38	i. O.
	14	5	ii. *t*.
	16	59	ii. T.
	22	40	i. *t*.
11	0	59	i. T.
	17	2	iii. *t*.
	18	33	i. *e*.
	20	33	iii. T.
	22	7	i. O.
12	6	43	ii. *e*.
	12	9	ii. O.
	17	9	i. *t*.
	19	28	i. T.
13	13	1	i. *e*.
	16	36	i. O.
14	3	26	ii. *t*.
	6	20	ii. T.
	11	38	i. *t*.
	13	57	i. T.
15	1	47	iii. *e*.
	5	14	iii. E.
	7	2	iii. *o*.
	7	30	i. *e*.
	10	32	iii. O.
	11	6	i. O.

May (con.).

d	h	m	
15	20	0	ii. *e*.
16	1	29	ii. O.
	6	7	i. *t*.
	8	26	i. T.
17	1	58	i. *e*.
	5	35	i. O.
	6	14	iv. *e*.
	10	40	iv. E.
	16	47	ii. *t*.
	18	40	iv. *o*.
	19	41	ii. T.
	22	57	iv. O.
18	0	37	i. *t*.
	2	56	i. T.
	20	27	i. *e*.
	21	15	iii. *t*.
19	0	4	i. O.
	0	44	iii. T.
	9	18	ii. *e*.
	14	49	ii. O.
	19	6	i. *t*.
	21	25	i. T.
20	14	55	i. *e*.
	18	33	i. O.
21	6	7	ii. *t*.
	9	1	ii. T.
	13	35	i. *t*.
	15	54	i. T.
22	5	47	iii. *e*.
	9	14	iii. E.
	9	24	i. *e*.
	11	13	iii. *o*.
	13	2	i. O.
	14	41	iii. O.
	22	35	ii. *e*.
23	4	9	ii. O.
	8	3	i. *t*.
	10	22	i. T.
24	3	52	i. *e*.
	7	31	i. O.
	19	28	ii. *t*.
	22	21	ii. T.
25	2	33	i. *t*.
	4	51	i. T.
	22	21	i. *e*.
26	1	24	iii. *t*.
	2	0	i. O.
	4	51	iii. T.
	5	34	iv. *t*.
	9	45	iv. T.
	11	53	ii. *e*.
	17	28	ii. O.

May (con.).

d	h	m	
26	21	1	i. *t*.
	23	20	i. T.
27	16	49	i. *e*.
	20	29	i. O
28	8	47	ii. *t*
	11	40	ii. T
	15	30	i. *t*.
	17	49	i. T
29	9	48	iii.
	11	18	i. *e*
	13	14	iii.
	14	58	i. O
	15	21	iii.
	18	48	iii.
30	1	10	ii.
	6	46	ii. O
	9	59	i. *t*.
	12	18	i. T.
31	5	46	i. *e*.
	9	26	i. O
	22	7	ii. *t*.

June.

d	h	m	
1	1	0	ii. T
	4	27	i. *t*.
	6	46	i. T.
2	0	15	i. *e*.
	3	55	i. O
	5	29	iii.
	8	55	iii. T
	14	28	ii. *e*
	20	4	ii. O
	22	56	i. *t*.
3	0	26	iv.
	1	15	i. T.
	4	48	iv.
	13	33	iv. *e*
	17	37	iv. O
	18	43	i. *e*.
	22	24	i. O.
4	11	25	ii. *t*.
	14	18	ii. T
	17	25	i. *t*.
	19	43	i. T
5	13	12	i. *e*.
	13	48	iii
	16	52	i.
	17	14	iii
	19	25	iii
	22	50	ii

).
. *e*.
. O.
t.
T.
e.
O.
.. *t*.
.. T.
. *t*.
. T.
. *e*.
. O.
ii. *t*.
ii. T.
i. *e*.
ii. O.
i. *t*.
i. T.
i. *e*.
i. O.
ii. *t*.
ii. T.
i. *t*.
i. T.
iv. *t*.
iv. T.
i. *e*.
iii. *e*.
i. O.
iii. E.
iii. *o*.
iii. O.
i. *e*.
ii. O.
i. *t*.
i. T.
i. *e*.
i. O.
ii. *t*.
ii. T.
5 i. *t*.
3 i. T.
3 i. *e*.
2 i. O.
9 iii. *t*.
2 iii. T.
8 ii. *e*.
1 ii. O.
3 i. *t*.
1 i. T.
1 i. *e*.
1 i. O.
6 ii. *t*.
8 ii. T.
1 i. *t*.
9 i. T.
0 i. *e*.
8 iv. *e*.
9 i. O.
iii. *e*.

June (con.).

d	h	m	
20	1	13	iii. E.
	3	21	iii. *o*.
	6	43	iii. O.
	7	36	iv. *o*.
	8	55	ii. *e*.
	11	27	iv. O.
	14	27	ii. O.
	15	39	i. *t*.
	17	57	i. T.
21	11	28	i. *e*.
	15	7	i. O.
22	5	53	ii. *t*.
	8	45	ii. T.
	10	7	i. *t*.
	12	25	i. T.
23	5	57	i. *e*.
	9	35	i. O.
	17	23	iii. *t*.
	20	44	iii. T.
	22	12	ii. *e*.
24	3	42	ii. O.
	4	35	i. *t*.
	6	53	i. T.
25	0	25	i. *e*.
	4	3	i. O.
	19	8	ii. *t*.
	22	0	ii. T.
	23	2	i. *t*.
26	1	20	i. T.
	18	54	i. *e*.
	22	30	i. O.
27	1	49	iii. *e*.
	5	13	iii. E.
	7	11	iii. *o*.
	10	32	iii. O.
	11	29	ii. *e*.
	16	56	ii. O.
	17	30	i. *t*.
	19	48	i. T.
28	13	22	i. *e*.
	16	58	i. O.
	17	41	iv. *t*.
	21	26	iv. T.
29	8	24	ii. *t*.
	11	16	ii. T.
	11	58	i. *t*.
	14	16	i. T.
30	7	51	i. *e*.
	11	26	i. O.
	21	11	iii. *t*.

July.

d	h	m	
1	0	31	iii. T.
	0	47	ii. *e*.
	6	10	ii. O.
	6	25	i. *t*.
	8	43	i. T.
2	2	20	i. *e*.
	5	54	i. O.

July (con.).

d	h	m	
2	21	39	ii. *t*.
3	0	30	ii. T.
	0	53	i. *t*.
	3	11	ii. T.
	20	48	i. *e*.
4	0	21	i. O.
	5	50	iii. *e*.
	9	13	iii. E.
	10	58	iii. *o*.
	14	4	ii. *e*.
	14	18	iii. O.
	19	20	i. *t*.
	19	23	ii. O.
	21	38	i. T.
5	15	17	i. *e*.
	18	49	i. O.
6	10	54	ii. *t*.
	12	51	iv. *e*.
	13	45	ii. T.
	13	48	i. *t*.
	16	6	i. T.
	17	3	iv. E.
7	0	42	iv. *o*.
	4	22	iv. O.
	9	45	i. *e*.
	13	16	i. O.
8	0	55	iii. *t*.
	3	21	ii. *e*.
	4	14	iii. T.
	8	15	i. *t*.
	8	36	ii. O.
	10	33	i. T.
9	4	14	i. *e*.
	7	43	i. O.
10	0	7	ii. *t*.
	2	42	i. *t*.
	2	58	ii. T.
	5	0	i. T.
	22	42	i. *e*.
11	2	11	i. O.
	9	51	iii. *e*.
	13	13	iii. E.
	14	40	iii. *o*.
	16	39	ii. *e*.
	17	58	iii. O.
	21	9	i. *t*.
	21	48	ii. O.
	23	27	i. T.
12	17	11	i. *e*.
	20	38	i. O.
13	13	21	ii. *t*.
	15	36	i. *t*.
	16	12	ii. T.
	17	54	i. T.
14	11	40	i. *e*.
	15	5	i. O.
15	4	34	iii. *t*.
	5	56	ii. *e*.
	7	52	iii. T.
	10	3	i. *t*.
	10	18	iv. *t*.

July (con.).

d	h	m	
15	11	0	ii. O.
	12	21	i. T.
	13	53	iv. T.
16	6	8	i. *e*.
	9	32	i. O.
17	2	33	ii. *t*.
	4	30	i. *t*.
	5	24	ii. T.
	6	48	i. T.
18	0	37	i. *e*.
	3	59	i. O.
	13	52	iii. *e*.
	17	14	iii. E.
	18	17	iii. *o*.
	19	13	ii. E.
	21	35	iii. O.
	22	57	i. *t*.
19	0	11	ii. O.
	1	15	i. T.
	19	5	i. *e*.
	22	26	i. O.
20	15	46	ii. *t*.
	17	24	i. *t*.
	18	36	ii. T.
	19	42	i. T.
21	13	34	i. *e*.
	16	53	i. O.
22	8	8	iii. *t*.
	8	30	ii. *e*.
	11	25	iii. T.
	11	51	i. *t*.
	13	21	ii. O.
	14	9	i. T.
23	7	4	iv. *e*.
	8	2	i. *e*.
	11	11	iv. E.
	11	20	i. O.
	16	48	iv. *o*.
	20	18	iv. O.
24	4	57	ii. *t*.
	6	17	i. *t*.
	7	48	ii. T.
	8	35	i. T.
25	2	31	i. *e*.
	5	47	i. O.
	17	53	iii. *e*.
	21	14	iii. E.
	21	48	ii. *e*.
	21	50	iii. *o*.
26	0	44	i. *t*.
	1	7	iii. O.
	2	31	ii. O.
	3	2	i. T.
	21	0	i. *e*.
27	0	14	i. O.
	18	9	ii. *t*.
	19	10	i. *t*.
	21	0	ii. T.
	21	28	i. T.
28	15	28	i. *e*.
	18	40	i. O.

July (con.).

d	h	m	
29	11	5	ii. *e*.
	11	38	iii. *t*.
	13	37	i. *t*.
	14	55	iii. T.
	15	41	ii. O.
	15	55	i. T.
30	9	57	i. *e*.
	13	7	i. O.
31	7	19	ii. *t*.
	8	3	i. *t*.
	10	10	ii. T.
	10	21	i. T.

August.

d	h	m	
1	1	56	iv. *t*.
	4	26	i. *e*.
	5	25	iv. T.
	7	33	i. O.
	21	54	iii. *e*.
2	0	22	ii. *e*.
	1	14	iii. E.
	1	17	iii. *o*.
	2	30	i. *t*.
	4	34	iii. O.
	4	48	i. T.
	4	50	ii. O.
	22	54	i. *e*.
3	2	0	i. O.
	20	30	ii. *t*.
	20	56	i. *t*.
	23	14	i. T.
	23	21	ii. T.
4	17	23	i. *e*.
5	20	26	i. O.
	13	40	ii. *e*.
	15	3	iii. *t*.
	15	22	i. *t*.
	17	40	i. T.
	17	58	ii. O.
	18	20	iii. T.
6	11	52	i. *e*.
	14	53	i. O.
7	9	40	ii. *t*.
	9	49	i. *t*.
	12	6	i. T.
	12	30	ii. E.
8	6	20	i. *e*.
	9	19	i. O.
9	1	18	iv. *e*.
	1	55	iii. *e*.
	2	57	ii. *e*.
	4	15	i. *t*.
	5	20	iv. E.
	6	33	i. T.
	7	6	ii. O.
	7	56	iv. *o*.
	7	58	iii. O.
	11	24	iv. O.
10	0	49	i. *e*.

Aug. (con

d	h	m	
10	3	45	i.
	22	41	i.
	22	50	ii
11	0	59	i.
	1	40	ii
	19	18	i.
	22	12	i.
12	16	14	ii
	17	7	i.
	18	26	ii
	19	25	i.
	20	14	ii
	21	43	ii
13	13	46	i.
	16	38	i.
14	11	33	i.
	11	58	ii
	13	51	i.
	14	49	ii
15	8	15	i.
	11	4	i.
16	5	32	ii
	5	56	ii
	5	59	i.
	8	17	i.
	9	21	ii
	11	19	ii
17	2	44	i.
	5	30	i.
	16	43	iv
	20	12	iv
18	0	25	i.
	1	8	ii
	2	43	i.
	3	58	ii
	21	12	i.
	23	56	i.
19	18	49	ii
	18	51	i.
	21	9	i.
	21	45	ii
	22	29	ii
20	1	2	ii
	15	41	i.
	18	22	i.
21	13	17	i.
	14	16	ii
	15	35	i.
	17	6	ii
22	10	10	i.
	12	48	i.
23	7	43	i.
	8	7	ii
	9	57	ii
	10	0	i.
	11	36	ii
	14	37	ii
24	4	38	i.
	7	14	i.
25	2	8	i
	3	25	

Aug. (con.).

d	h	m	
25	4	26	i. T.
	6	15	ii. T.
	19	33	iv. *e*.
	23	7	i. *e*.
26	1	40	i. O.
	1	50	iv. O.
	20	34	i. *t*.
	21	24	ii. *e*.
	22	52	i. T.
27	0	42	ii. O.
	1	1	iii. *t*.
	4	19	iii. T.
	17	36	i. *e*.
	20	6	i. O.
28	15	0	i. *t*.
	16	32	ii. *t*.
	17	18	i. T.
	19	23	ii. T.
29	12	5	i. *e*.
	14	32	i. O.
30	9	26	i. *t*.
	10	42	ii. *e*.
	11	44	i. T.
	13	49	ii. O.
	13	58	iii. *e*.
	17	54	iii. O.
31	6	33	i. *e*.
	8	58	i. O.

September.

d	h	m	
1	3	52	i. *t*.
	5	41	ii. *t*.
	6	9	i. T.
	8	31	ii. T.
2	1	2	i. *e*.
	3	24	i. O.
	22	17	i. *t*.
	23	59	ii. *e*.
3	0	35	i. T.
	2	56	ii. O.
	4	16	iii. *t*.
	6	58	iv. *t*.
	7	35	iii. T.
	10	33	iv. T.
	19	31	i. *e*.
	21	50	i. O.
4	16	43	i. *t*.
	18	48	ii. *t*.
	19	1	i. T.
	21	39	ii. T.
5	13	58	i. *e*.
	16	16	i. O.
6	11	9	i. *t*.
	13	11	ii. *o*.
	13	27	i. T.
	16	3	ii. E.
	17	51	iii. *o*.
	21	17	iii. E.
7	8	24	i. *o*.

Sept. (con.).

d	h	m	
7	10	43	i. E.
8	5	35	i. *t*.
	7	53	i. T.
	7	57	ii. *t*.
	10	48	ii. T.
9	2	50	i. *o*.
	5	12	i. E.
10	0	1	i. *t*.
	2	18	ii. *o*.
	2	19	i. T.
	5	21	ii. E.
	7	31	iii. *t*.
	10	51	iii. T.
	21	16	i. *o*.
	23	40	i. E.
11	12	23	iv. *o*.
	17	38	iv. E.
	18	26	i. *t*.
	20	44	i. T.
	21	4	ii. *t*.
	23	55	ii. T.
12	15	42	i. *o*.
	18	9	i. E.
13	12	52	i. *t*.
	15	10	i. T.
	15	24	ii. *o*.
	18	38	ii. E.
	21	7	iii. *o*.
14	1	18	iii. E.
	10	8	i. *o*.
	12	38	i. E.
15	7	18	i. *t*.
	9	36	i. T.
	10	13	ii. *t*.
	13	4	ii. T.
16	4	34	i. *o*.
	7	7	i. E.
17	1	44	i. *t*.
	4	2	i. T.
	4	31	ii. *o*.
	7	56	ii. E.
	10	47	iii. *t*.
	14	8	iii. T.
	23	0	i. *o*.
18	1	36	i. E.
	20	10	i. *t*.
	22	28	i. T.
	23	21	ii. *t*.
19	2	12	ii. T.
	17	27	i. *o*.
	20	5	i. E.
	21	0	iv. *t*.
20	0	52	iv. T.
	14	50	i. *t*.
	16	54	i. T.
	17	58	ii. *o*.
	21	14	ii. E.
21	0	24	iii. *o*.
	5	19	iii. E.
	11	53	i. *o*.
	14	33	i. E.

Sept. (con.).

d	h	m	
22	9	2	i. *t*.
	11	20	i. T.
	12	30	ii. *t*.
	15	21	ii. T.
23	6	19	i. *o*.
	9	2	i. E.
24	3	29	i. *t*.
	5	47	i. T.
	6	45	ii. *o*.
	10	31	ii. E.
	14	5	iii. *t*.
	17	27	iii. T.
25	0	45	i. *o*.
	3	31	i. E.
	21	55	i. *t*.
26	0	13	i. T.
	1	38	ii. *t*.
	4	30	ii. T.
	19	12	i. *o*.
	22	0	i. E.
27	16	21	i. *t*.
	18	39	i. T.
	19	53	ii. *o*.
	23	49	ii. E.
28	2	38	iv. *o*.
	3	42	iii. *o*.
	6	26	iv. O.
	8	6	iv. *e*.
	9	20	iii. E.
	11	48	iv. E.
	13	38	i. *o*.
	16	29	i. E.
29	10	47	i. *t*.
	13	5	i. T.
	14	47	ii. *t*
	17	39	ii. T.
30	8	4	i. *o*.
	10	57	i. E.

October.

d	h	m	
1	5	13	i. *t*.
	7	31	i. T.
	9	1	ii *o*.
	13	7	ii. E.
	17	25	iii. *t*.
	20	48	iii. T.
2	2	31	i. *o*.
	5	26	i. E.
	23	40	i. *t*.
3	1	58	i. T.
	3	57	ii. *t*.
	6	40	ii. T.
	20	57	i. *o*.
	23	55	i. E.
4	18	6	i. *t*.
	20	24	i. T.
	22	10	ii. *o*.
5	2	25	ii. E.
	7	3	iii. *o*.

Oct. (con.).

d	h	m	
5	13	21	iii. E.
	15	24	i. *o*.
	18	24	i. E.
6	11	44	iv. *t*.
	12	33	i. *t*.
	14	51	i. T.
	15	36	iv. T.
	17	7	ii. *t*.
	19	59	ii. T.
7	9	50	i. *o*.
	12	53	i. E.
8	6	59	i. *t*.
	9	17	i. T.
	11	19	ii. *o*.
	15	43	ii. E.
	20	49	iii. *t*.
9	0	13	iii. T.
	4	17	i. *o*.
	7	22	i. E.
10	1	26	i. *t*.
	3	44	i. T.
	6	17	ii. *t*.
	9	9	ii. T.
	22	44	i. *o*.
11	1	51	i. E.
	19	52	i. *t*.
	22	11	i. T.
12	0	29	ii. *o*.
	5	1	ii. E.
	10	30	iii. *o*.
	13	54	iii. O.
	14	10	iii. *e*.
	17	11	i. *o*.
	17	22	iii. E.
	20	19	i. E.
13	14	19	i. *t*.
	16	38	i. T.
	19	28	ii. *t*.
	22	21	ii. T.
14	11	38	i. *o*.
	14	48	i. E.
	17	31	iv. *o*.
	21	28	iv. O.
15	2	25	iv. *e*.
	5	58	iv. E.
	8	46	i. *t*.
	11	4	i. T.
	13	39	ii. *o*.
	18	19	ii. E.
16	0	17	iii. *t*.
	3	42	iii. T.
	6	5	i. *o*.
	9	17	i. E.
17	3	13	i. *t*.
	5	31	i. T.
	8	50	ii. *t*.
	11	32	ii. T.
18	0	32	i. *o*.
	3	46	i. E.
	21	40	i. *t*.
	23	58	i. T.

Oct. (con.).

d	h	m	
19	2	49	ii. *o*.
	7	37	ii. E.
	14	0	iii. *o*.
	17	26	iii. O.
	18	13	iii. *e*.
	18	59	i. *o*.
	21	23	iii. E.
	22	15	i. E.
20	16	7	i. *t*.
	18	26	i. T.
	21	51	ii. *t*.
21	0	44	ii. T.
	13	26	i. *o*.
	16	44	i. E.
22	10	34	i. *t*.
	12	53	i. T.
	16	1	ii. *o*.
	20	56	ii. E.
23	3	6	iv. *t*.
	3	50	iii. *t*.
	7	7	iv. T.
	7	16	iii. T.
	7	53	i. *o*.
	11	13	i. E.
24	5	1	i. *t*.
	7	20	i. T.
	11	3	ii. *t*.
	13	56	ii. T.
25	2	21	i. *o*.
	5	42	i. E.
	23	29	i. *t*.
26	1	47	i. T.
	5	13	ii. *o*.
	10	14	ii. E.
	17	36	iii. *o*.
	20	48	i. *o*.
	21	3	iii. O.
	22	16	iii. *e*.
27	0	11	i. E.
	1	25	iii. E.
	17	56	i. *t*.
	20	15	i. T.
28	0	17	ii. *t*.
	3	10	ii. T.
	15	15	i. *o*.
	18	40	i. E.
29	12	23	i. *t*.
	14	42	i. T.
	18	25	ii. *o*.
	23	32	ii. E.
30	7	27	iii. *t*.
	9	43	i. *o*.
	10	54	iii. T.
	13	8	i. E.
31	6	51	i. *t*.
	9	10	i. T.
	9	20	iv. *o*.
	13	25	iv. O.
	13	30	ii. *t*.
	16	23	ii. T.
	20	44	iv. *e*.

d
1
2
3
4
5
6
7
8
9
10
11
12
13
14

Nov. (con.).

d	h	m	
14	18	31	ii. *t.*
	21	24	ii. T.
15	7	55	i. *o.*
	11	29	i. E.
16	5	3	i. *t.*
	7	21	i. T.
	12	38	ii. *o.*
	18	4	ii. E.
17	2	14	iv. *o.*
	2	23	i. *o.*
	4	53	iii. *o.*
	5	58	i. E.
	6	23	iv. O.
	8	21	iii. O.
	10	23	iii. *e.*
	13	30	iii. E.
	15	4	iv. *e.*
	18	20	iv. E.
	23	31	i. *t.*
18	1	50	i. T.
	7	47	ii. *t.*
	10	41	ii. T.
	20	51	i. *o.*
19	0	27	i. E.
	17	59	i. *t.*
	20	18	i. T.
20	1	55	ii. *o.*
	7	23	ii. E.
	15	20	i. *o.*
	18	52	iii. *t.*
	18	55	i. E.
	22	20	iii. T.
21	12	28	i. *t.*
	14	47	i. T.
	21	4	ii. *t.*
	23	57	ii. T.
22	9	48	i. *o.*

Nov. (con.).

d	h	m	
22	13	24	i. E.
23	6	56	i. *t.*
	9	15	i. T.
	15	12	ii. *o.*
	20	42	ii. E.
24	4	17	i. *o.*
	7	53	i. E.
	8	48	iii. *o.*
	12	16	iii. O.
	14	25	iii. *e.*
	17	31	iii. E.
25	1	25	i. *t.*
	3	44	i. T.
	10	22	ii. *t.*
	12	55	iv. *t.*
	13	15	ii. T.
	17	5	iv. T.
	22	46	i. *o.*
26	2	22	i. E.
	19	54	i. *t.*
	22	13	i. T.
27	4	30	ii. *o.*
	10	0	ii. E.
	17	15	i. *o.*
	20	51	i. E.
	22	50	iii. *t.*
28	2	18	iii. T.
	14	23	i. *t.*
	16	42	i. T.
	23	40	ii. *t.*
29	2	33	ii. T.
	11	44	i. *o.*
	15	20	i. E.
30	8	52	i. *t.*
	11	10	i. T.
	17	48	ii. *o.*
	23	19	ii. E.

December.

d	h	m	
1	6	12	i. *o.*
	9	49	i. E.
	12	48	iii. *o.*
	16	17	iii. O.
	18	28	iii. *e.*
	21	33	iii. E.
2	3	21	i. *t.*
	5	39	i. T.
	12	58	ii. *t.*
	15	51	ii. T.
3	0	41	i. *o.*
	4	18	i. E.
	20	10	iv. *o.*
	21	50	i. *t.*
4	0	8	i. T.
	0	21	iv. O.
	7	7	ii. *o.*
	9	25	iv. *e.*
	12	31	iv. E.
	12	38	ii. E.
	19	10	i. *o.*
	22	47	i. E.
5	2	53	iii. *t.*
	6	21	iii. T.
	16	19	i. *t.*
	18	37	i. T.
6	2	17	ii. *t.*
	5	10	ii. T.
	13	40	i. *o.*
	17	16	i. E.
7	10	48	i. *t.*
	13	7	i. T.
	20	26	ii. *o.*
8	1	57	ii. E.
	8	9	i. *o.*
	11	44	i. E.

Dec. (con.).

d	h	m	
8	16	53	iii. *o.*
	20	21	iii. O.
	22	31	iii. *e.*
9	1	34	iii. E.
	5	17	i. *t.*
	7	36	i. T.
	15	37	ii. *t.*
	18	30	ii. T.
10	2	38	i. *o*
	6	13	i. E.
	23	46	i. *t.*
11	2	5	i. T.
	9	46	ii. *o.*
	15	15	ii. E.
	21	7	i. *o.*
12	0	42	i. E.
	6	59	iii. *t.*
	7	20	iv. *t.*
	10	27	iii. T.
	11	29	iv. T.
	18	16	i. *t.*
	20	34	i. T.
13	4	57	ii. *t.*
	7	50	ii. T.
	15	36	i. *o.*
	19	11	i. E.
14	12	45	i. *t.*
	15	4	i. T.
	23	7	ii. *o.*
15	4	35	ii. E.
	10	6	i. *o.*
	13	40	i. E.
	21	3	iii. *o.*
16	0	30	iii. O.
	2	34	iii. *e.*
	5	36	iii. E.

Dec. (con.).

d	h	m	
16	7	14	i. *t.*
	9	33	i. T.
	18	17	ii. *t.*
	21	11	ii. T.
17	4	35	i. *o.*
	8	9	i. E.
18	1	44	i. *t.*
	4	3	i. T.
	12	28	ii. *o.*
	17	53	ii. E.
	23	5	i. *o.*
19	2	38	i. E.
	11	10	iii. *t.*
	14	38	iii. T.
	20	13	i. *t.*
	22	32	i. T.
20	7	38	ii. *t.*
	10	31	ii. T.
	15	2	iv. *o.*
	17	34	i. *o.*
	19	10	iv. O.
	21	7	i. E.
21	3	46	iv. *e.*
	6	41	iv. E.
	14	43	i. *t.*
	17	2	i. T.
22	1	49	ii. *o.*
	7	12	ii. E.
	12	4	i. *o.*
	15	36	i. E.
23	1	15	iii. *o.*
	4	43	iii. O.
	6	37	iii. *e.*
	9	13	i. *t.*
	9	38	iii. E.
	11	32	i. T.

Dec. (co

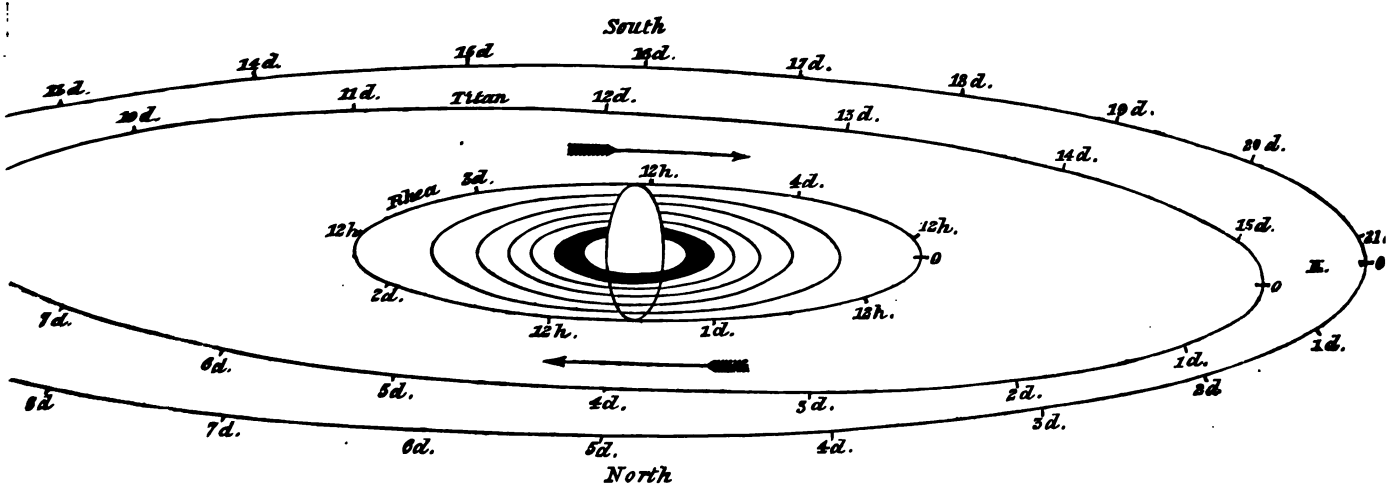
d	h	m
23	21	0
	23	53
24	6	34
	10	4
25	3	42
	6	1
	15	11
	20	31
26	1	3
	4	33
	15	25
	18	52
	22	12
27	0	31
	10	21
	13	14
	19	33
	23	2
28	16	42
	19	1
29	2	36
	4	34
	6	42
	9	50
	14	3
	17	31
30	5	31
	8	57
	10	39
	11	12
	13	31
	13	39
	23	43
31	2	36
	8	33
	12	0

Satellites I. and II. *disappear* at eclipse on the *p* side of the planet until September 4, the distance between each satellite and the planet at disappearance being nearly the same till the end of July, when it rapidly decreases. Both satellites *reappear* on the *f* side after September 4.

Satellite III. *disappears* and *reappears* *p* until July 25. The *disappearance* only is seen *p* from August 9 to 30, and the *reappearance* only *f* from August 30 to October 5; from October 12 the satellite *disappears* and *reappears* on the *f* side. The eclipses of III. on August 2 and October 12 are interesting, the point of reappearance being very near the intersection with the shadow, so that the satellite will disappear again in about 3^m and 16^m respectively.

Satellite IV. both *disappears* and *reappears* on the *p* side until August 9, on August 25 the *disappearance* only is visible *f*, and on September 11 the *reappearance* only *f*: from September 28 it *disappears* and *reappears* on the *f* side.

T ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

(The Vertical Scale is twice the Horizontal.)

SATELLITES OF SATURN.

East Elongations.

MIMAS.

January.	February.	March.	April.	May.	June.	December.
d h	d h	d h	d h	d h	d h	d h
1 9·1	1 11·4	1 18·0	1 20·4	1 1·6	1 4·0	1 0·6
2 7·7	2 10·0	2 16·6	2 19·0	2 0·2	2 2·6	1 23·2
3 6·3	3 8·6	3 15·2	3 17·6	2 22·8	3 1·3	2 21·9
4 4·9	4 7·2	4 13·9	4 16·3	3 21·4	3 23·9	3 20·5
5 3·5	5 5·9	5 12·5	5 14·9	4 20·0	4 22·5	4 19·1
6 2·1	6 4·5	6 11·1	6 13·5	5 18·7	5 21·1	5 17·7
7 0·7	7 3·1	7 9·7	7 12·1	6 17·3	6 19·7	6 16·3
7 23·3	8 1·7	8 8·3	8 10·7	7 15·9	7 18·4	7 15·0
8 22·0	9 0·4	9 7·0	9 9·4	8 14·5	8 17·0	8 13·6
9 20·6	9 23·0	10 5·6	10 8·0	9 13·1	9 15·6	9 12·2
10 19·2	10 21·6	11 4·2	11 6·6	10 11·7	10 14·2	10 10·8
11 17·9	11 20·3	12 2·8	12 5·2	11 10·3	11 12·8	11 9·4
12 16·5	12 18·9	13 1·4	13 3·8	12 9·0	12 11·4	12 8·1
13 15·1	13 17·5	14 0·0	14 2·4	13 7·6	13 10·0	13 6·7
14 13·7	14 16·1	14 22·6	15 1·0	14 6·2		14 5·3
15 12·3	15 14·7	15 21·2	15 23·7	15 4·8		15 3·9
16 10·9	16 13·4	16 19·9	16 22·3	16 3·5	**November.**	16 2·5
17 9·5	17 12·1	17 18·5	17 20·9	17 2·1		17 1·1
18 8·1	18 10·7	18 17·1	18 19·6	18 0·7	16 21·4	17 23·7
19 6·7	19 9·3	19 15·8	19 18·2	18 23·3	17 20·0	18 22·4
20 5·3	20 7·9	20 14·4	20 16·8	19 21·9	18 18·6	19 21·0
21 4·0	21 6·5	21 13·0	21 15·4	20 20·6	19 17·2	20 19·6
22 2·6	22 5·1	22 11·6	22 14·0	21 19·2	20 15·9	21 18·2
23 1·3	23 3·7	23 10·2	23 12·6	22 17·8	21 14·5	22 16·8
23 23·9	24 2·4	24 8·8	24 11·3	23 16·4	22 13·1	23 15·5
24 22·5	25 1·0	25 7·5	25 9·9	24 15·0	23 11·7	24 14·1
25 21·1	25 23·6	26 6·1	26 8·5	25 13·7	24 10·3	25 12·7
26 19·7	26 22·2	27 4·7	27 7·1	26 12·3	25 8·9	26 11·3
27 18·4	27 20·8	28 3·3	28 5·7	27 10·9	26 7·6	27 10·0
28 17·0	28 19·4	29 1·9	29 4·4	28 9·5	27 6·2	28 8·6
29 15·6		30 0·5	30 3·0	29 8·2	28 4·8	29 7·2
30 14·2		30 23·1		30 6·8	29 3·4	30 5·8
31 12·8		31 21·8		31 5·4	30 2·0	31 4·4

ENCELADUS.

January.	February.	March.	April.	May.	November.	December.
d h	d h	d h	d h	d h	d h	d h
1 12·8	2 1·1	1 10·8	1 23·2	2 2·6	1 18·2	1 21·7
2 21·6	3 10·0	2 19·7	3 8·1	3 11·5	3 3·1	3 6·5
4 6·5	4 18·9	4 4·6	4 16·9	4 20·4	4 11·9	4 15·4
5 15·4	6 3·8	5 13·5	6 1·8	6 5·3	5 20·8	6 0·3
7 0·3	7 12·7	6 22·4	7 10·7	7 14·2	7 5·7	7 9·2
8 9·2	8 21·5	8 7·2	8 19·6	8 23·1	8 14·6	8 18·1
9 18·0	10 6·4	9 16·1	10 4·5	10 7·9	9 23·5	10 3·0
11 2·9	11 15·3	11 1·0	11 13·4	11 16·8	11 8·4	11 11·9
12 11·8	13 0·2	12 9·9	12 22·3	13 1·7	12 17·3	12 20·7
13 20·7	14 9·1	13 18·8	14 7·1	14 10·6	14 2·1	14 5·6
15 5·6	15 18·0	15 3·7	15 16·0	15 19·5	15 11·0	15 14·5
16 14·5	17 2·9	16 12·6	17 0·9	17 4·4	16 19·9	16 23·4
17 23·4	18 11·8	17 21·4	18 9·8	18 13·2	18 4·8	18 8·3
19 8·3	19 20·6	19 6·3	19 18·7	19 22·1	19 13·7	19 17·2
20 17·1	21 5·5	20 15·2	21 3·6	21 7·0	20 22·6	21 2·1
22 2·0	22 14·4	22 0·1	22 12·5	22 15·9	22 7·5	22 10·9
23 10·9	23 23·3	23 9·0	23 21·3	24 0·8	23 16·4	23 19·8
24 19·8	25 8·2	24 17·9	25 6·2		25 1·2	25 4·7
26 4·7	26 17·0	26 2·7	26 15·1		26 10·1	26 13·6
27 13·5	28 1·9	27 11·6	28 0·0		27 19·0	27 22·5
28 22·4		28 20·5	29 8·9		29 3·9	29 7·4
30 7·3		30 5·4	30 17·8		30 12·8	30 16·2
31 16·2		31 14·3				

East Elongations.

TETHYS.

January.	February.	March.	April.	May.	June.	December.
d h	d h	d h	d h	d h	d h	d h
1 17·8	2 19·8	1 6·0	2 8·0	2 12·8	1 17·5	1 22·4
3 15·0	4 17·1	3 3·3	4 5·3	4 10·1	3 14·8	3 19·7
5 12·3	6 14·4	5 0·6	6 2·6	6 7·4	5 12·1	5 17·0
7 9·6	8 11·7	6 21·9	7 23·9	8 4·7	7 9·4	7 14·3
9 6·9	10 9·0	8 19·2	9 21·2	10 2·0	9 6·7	9 11·6
11 4·2	12 6·3	10 16·5	11 18·5	11 23·3	11 4·0	11 8·9
13 1·6	14 3·6	12 13·8	13 15·8	13 20·5	13 1·3	13 6·2
14 22·8	16 0·9	14 11·0	15 13·1	15 17·8		15 3·5
16 20·1	17 22·2	16 8·3	17 10·4	17 15·1	November.	17 0·8
18 17·4	19 19·5	18 5·6	19 7·7	19 12·4		18 22·1
20 14·7	21 16·8	20 2·9	21 5·0	21 9·7	18 17·2	20 19·4
22 12·0	23 14·0	22 0·2	23 2·3	23 7·0	20 14·5	22 16·7
24 9·3	25 11·4	23 21·5	24 23·6	25 4·3	22 11·8	24 14·0
26 6·6	27 8·7	25 18·8	26 20·9	27 1·6	24 9·2	26 11·4
28 3·9		27 16·1	28 18·2	28 22·9	26 6·5	28 8·7
30 1·2		29 13·4	30 15·5	30 20·2	28 3·8	30 6·0
31 22·5		31 10·7			30 1·1	

DIONE.

January.	February.	March.	April.	May.	November.	December.
d h	d h	d h	d h	d h	d h	d h
3 6·1	2 8·4	1 17·0	3 12·9	3 15·1	16 19·6	3 5·6
5 23·8	5 2·0	4 10·6	6 6·5	6 8·8	19 13·2	5 23·2
8 17·5	7 19·7	7 4·3	9 0·2	9 2·5	22 6·9	8 16·9
11 11·1	10 13·4	9 22·0	11 17·9	11 20·1	25 0·6	11 10·6
14 4·8	13 7·0	12 15·6	14 11·5	14 13·8	27 18·2	14 4·3
16 22·4	16 0·7	15 9·3	17 5·2	17 7·4	30 11·9	16 21·9
19 16·1	18 18·3	18 2·9	19 22·8	20 1·1		19 15·6
22 9·7	21 12·0	20 20·6	22 16·5	22 18·8		22 9·3
25 3·4	24 5·7	23 14·2	25 10·2	25 12·4		25 3·0
27 21·1	26 23·3	26 7·9	28 3·8	28 6·1		27 20·6
30 14·7		29 1·6	30 21·5	30 23·7		30 14·3
		31 19·3				

RHEA.

January.	February.	March.	April.	May.	June.	December.
d h	d h	d h	d h	d h	d h	d h
4 18·9	5 9·4	4 11·6	5 2·1	2 4·3	2 18·8	5 4·5
9 7·3	9 21·8	8 23·9	9 14·5	6 16·6	7 7·2	9 17·1
13 19·6	14 10·1	13 12·3	14 2·8	11 5·0	11 19·6	14 5·7
18 8·0	18 22·5	18 0·7	18 15·2	15 17·4	16 7·9	18 18·3
22 20·3	23 10·9	22 13·0	23 3·5	20 5·8		23 6·9
27 8·7	27 23·2	27 1·4	27 15·9	24 18·1	November.	27 19·5
31 21·1		31 13·7		29 6·5	26 3·3	
					30 15·9	

TITAN.

January.	February.	March.	April.	May.	December.	
d h	d h	d h	d h	d h	d h	
1 17·6	2 15·6	6 10·8	7 5·7	9 1·4	3 1·7	......
17 16·9	18 13·5	22 8·2	23 3·5	25 0·0	19 1·1	......

East Elongations.

HYPERION.

January.	February.	March.	April.	May.	June.	November.	December.
d h	d h	d h	d h	d h	d h	d h	d h
14 20·0	5 7·3	19 10·4	9 15·2	22 3·5	12 12·3	9 1·0	21 18·2
	26 5·9		30 20·7			30 10·1	

IAPETUS.

	d h	d h	d h	d h	d h
Inf. Conjunction ...	Jan. 16 5·1,	Apr. 4 23·1,	June 23 8·1,	......	Dec. 2 14·6
W. Elongation	Feb. 4 14·7,	Apr. 23 20·6,	July 12 18·0,	Oct. 2 1·1,	Dec. 21 22·6.
Sup. Conjunction ...	Feb. 24 14·3,	May 14 1·7,	Aug. 2 11·1,	Oct. 22 20·3,	
E. Elongation	Mar. 17 1·0,	June 3 23·7,	......	Nov. 12 20·5.	

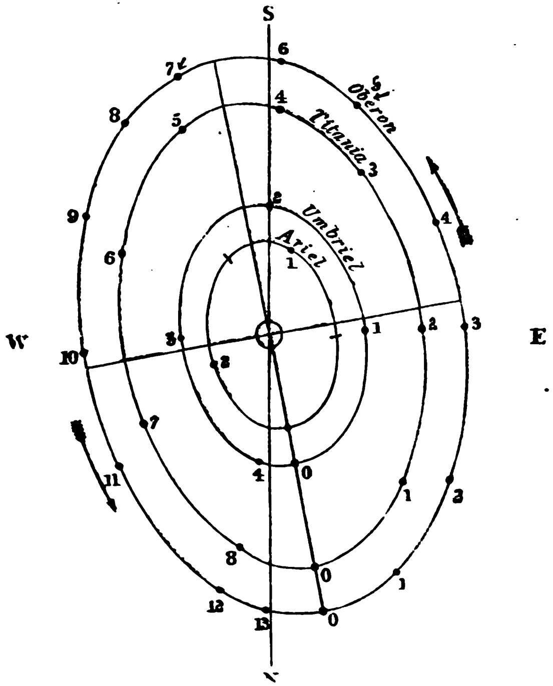
Apparent Elements of Saturn's Rings.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Latitude above Plane of Ring. Earth.	Latitude above Plane of Ring. Sun.
	° ′	″	″	° ′	° ′
Jan. 0	354 49·3	42·07	1·58	2 9·0 S.	4 42·2 S.
20	354 47·1	43·40	1·86	2 27·7 S.	4 23·6 S.
Feb. 9	354 42·0	44·39	2·36	3 3·2 S.	4 4·9 S.
Mar. 1	354 35·2	44·82	2·97	3 48·2 S.	3 46·3 S.
21	354 28·2	44·58	3·54	4 33·1 S.	3 27·7 S.
April 10	354 22·0	43·76	3·93	5 9·0 S.	3 9·1 S.
30	354 18·5	42·51	4·05	5 28·2 S.	2 50·5 S.
May 20	354 18·8	41·07	3·90	5 26·4 S.	2 31·8 S.
June 9	354 21·4	39·63	3·53	5 6·8 S.	2 13·2 S.
29	354 26·7	38·34	3·00	4 29·7 S.	1 54·6 S.
July 19	354 34·4	37·29	2·37	3 38·5 S.	1 35·9 S.
Aug. 8	354 44·0	36·54	1·67	2 37·2 S.	1 17·3 S.
28	354 55·1	36·10	0·94	1 29·5 S.	0 58·8 S.
Sept. 17	355 7·1	36·00	0·20	0 19·6 S.	0 40·2 S.
Oct. 7	355 19·2	36·23	0·51	0 48·4 N.	0 21·7 S.
27	355 30·8	36·80	1·18	1 50·2 N.	0 3·2 S.
Nov. 16	355 41·1	37·70	1·77	2 41·6 N.	0 15·3 N.
Dec. 6	355 49·0	38·87	2·24	3 18·4 N.	0 33·8 N.
26	355 53·7	40·25	2·54	3 37·4 N.	0 52·3 N.
31	355 54·4	40·61	2·58	3 39·1 N.	0 56·9 N.

The corrections to reduce to Mr. Marth's Ephemeris will be given in the Monthly numbers as soon as possible.

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 1·3	67	2 15·2	77	4 4·9		



Apparent Orbits of the Satellites of Uranus, as seen in an inverting telescope.

The small circle in the above diagram represents the planet, the arrows show the direction of motion, and the figures indicate the interval from the time of last North Elongation.

North Elongations.

ARIEL.

January.		February.		March.		April.		May.		June.		July.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
19	13·5	1	4·0	3	9·8	2	15·6	2	21·5	2	3·3	2	9·1
22	2·0	3	16·4	5	22·3	5	4·1	5	9·9	4	15·8	4	21·6
24	14·5	6	4·9	8	10·8	7	16·6	7	22·4	7	4·3	7	10·1
27	3·0	8	17·4	10	23·3	10	5·1	10	10·9	9	16·8	9	22·6
29	15·5	11	5·9	13	11·7	12	17·5	12	23·4	12	5·2	12	11·1
		13	18·4	16	0·2	15	6·0	15	11·9	14	17·7	14	23·6
		16	6·9	18	12·7	17	18·5	18	0·4	17	6·2	17	12·1
		18	19·3	21	1·2	20	7·0	20	12·8	19	18·7	20	0·5
		21	7·8	23	13·7	22	19·5	23	1·3	22	7·2	22	13·0
		23	20·3	26	2·2	25	8·0	25	13·8	24	19·7	25	1·5
		26	8·8	28	14·6	27	20·5	28	2·3	27	8·1	27	14·0
		28	21·3	31	3·1	30	9·0	30	14·8	29	20·6	30	2·4

UMBRIEL.

January. d h	February. d h	March. d h	April. d h	May. d h	June. d h	July. d h
13 16·8	3 10·1	4 10·4	2 10·6	1 10·8	3 14·6	2 14·9
17 20·3	7 13·5	8 13·8	6 14·0	5 14·3	7 18·1	6 18·4
21 23·8	11 17·0	12 17·3	10 17·5	9 17·8	11 21·6	10 21·8
26 3·2	15 20·5	16 20·8	14 21·0	13 21·3	16 1·0	15 1·3
30 6·7	20 0·0	21 0·2	19 0·5	18 0·7	20 4·5	19 4·7
	24 3·4	25 3·7	23 3·9	22 4·2	24 8·0	23 8·2
	28 6·9	29 7·2	27 7·4	26 7·7	28 11·5	27 11·7
				30 11·2		31 14·1

TITANIA.

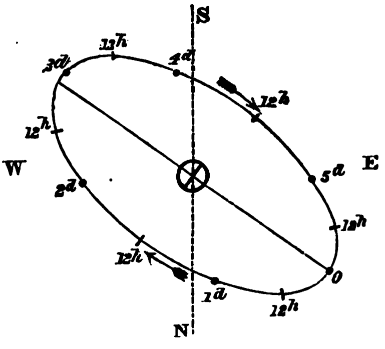
12 21·9	8 0·8	6 3·6	1 6·5	6 2·4	1 5·2	6 1·0
21 14·9	16 17·7	14 20·6	9 23·5	14 19·4	9 22·2	14 17·9
30 7·8	25 10·7	23 13·5	18 16·5	23 12·3	18 15·1	23 10·9
			27 9·5		27 8·1	32 3·8

OBERON.

18 8·4	14 6·7	13 5·0	8 3·4	6 1·8	2 0·2	......
31 19·6	27 17·9	26 16·2	22 14·6	19 13·0	15 11·4	......
					28 22·6	

Position-angle and distance of the apse of Oberon:—January 17, 9°·7, 43″·7 April 17, 10°·3, 46″·2; July 17, 10°·9, 43″·9.

SATELLITE OF NEPTUNE.



Apparent Orbit of the Satellite of Neptune, as seen in an inverting telescope.

The small circle in the above diagram represents the planet, the arrows show the direction of motion, and the figures indicate the interval from the time of last North-east Elongation.

North-east Elongations.

January. d h	February. d h	March. d h	September. d h	October. d h	November. d h	December. d h
2 4·8	6 11·1	1 23·3	5 23·3	5 8·5	3 17·7	3 3·0
8 1·9	12 8·1	7 20·3	11 20·3	11 5·6	9 14·8	9 0·0
13 22·9	18 5·2	13 17·3	17 17·4	17 2·6	15 11·8	14 21·0
19 19·9	24 2·2		23 14·4	22 23·6	21 8·9	20 18·1
25 17·0			29 11·5	28 20·7	27 5·9	26 15·1
31 14·0						

Position-angle and distance of the apse:—January 10, 239°·6, 16″·8; November 24, 242°·9, 17″·0.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1891.	P.	D.	L.	1891.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 1 49	−3 16	218 25	July 5	− 0 44	+3 31	296 33
6	− 0 37	3 50	152 34	10	+ 1 33	4 2	230 23
11	3 2	4 21	86 44	15	3 48	4 31	164 14
16	5 24	4 51	20 53	20	6 1	4 59	98 4
21	7 43	5 19	315 3	25	8 10	5 24	31 55
26	9 57	5 43	249 13	30	10 14	5 47	325 47
31	12 4	6 6	183 23	Aug. 4	12 14	6 8	259 39
Feb. 5	14 5	6 25	117 33	9	14 7	6 26	193 33
10	15 58	6 42	51 43	14	15 55	6 41	127 27
15	17 43	6 55	345 52	19	17 35	6 54	61 22
20	19 19	7 5	280 1	24	19 8	7 4	355 18
25	20 47	7 11	214 10	29	20 33	7 10	289 15
Mar. 2	22 5	7 15	148 18	Sept. 3	21 50	7 14	223 12
7	23 13	7 15	82 26	8	22 59	7 15	157 10
12	24 12	7 11	16 33	13	23 58	7 12	91 9
17	25 0	7 4	310 39	18	24 48	7 7	25 8
22	25 39	6 55	244 43	23	25 29	6 58	319 9
27	26 7	6 42	178 47	28	26 0	6 46	253 10
Apr. 1	26 24	6 26	112 49	Oct. 3	26 20	6 31	187 11
6	26 30	6 7	46 49	8	26 29	6 13	121 13
11	26 25	5 46	340 49	13	26 28	5 52	55 16
16	26 10	5 22	274 48	18	26 15	5 29	349 19
21	25 43	4 56	208 46	23	25 51	5 3	283 23
26	25 5	4 28	142 42	28	25 15	4 35	217 27
May 1	24 16	3 58	76 38	Nov. 2	24 27	4 4	151 30
6	23 16	3 26	10 32	7	23 27	3 32	85 35
11	22 6	2 53	304 25	12	22 16	2 58	19 40
16	20 46	2 19	238 17	17	20 53	2 22	313 45
21	19 15	1 44	172 8	22	19 19	1 46	247 51
26	17 36	1 9	105 59	27	17 34	1 8	181 57
31	15 48	−0 33	39 50	Dec. 2	15 40	+0 30	116 3
June 5	13 52	+0 4	333 39	7	13 37	−0 8	50 10
10	11 51	0 40	267 27	12	11 26	0 47	344 18
15	9 44	1 16	201 16	17	9 9	1 25	278 26
20	7 32	1 51	135 5	22	6 48	2 2	212 34
25	5 17	2 25	68 54	27	4 23	2 39	146 42
30	− 3 1	+2 59	2 43	32	+ 1 56	−3 14	80 50

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 18′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1890.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	T Cassiopeiæ	0 17 17	+55 11·0	53.	S Hydræ	8 47 50	+ 3 29·0
2.	R Andromedæ	0 18 13	+37 58·1	54.	T Hydræ	8 50 19	− 8 43·3
3.	S Ceti	0 18 28	− 9 56·3	55.	T Cancri	8 50 23	+20 16·1
4.	U Cassiopeiæ	0 40 13	+47 39·3	56.	S Antliæ	9 27 30	−28 8·6
5.	U Cephei	0 52 32	+81 16·9	57.	R Carini	9 29 28	−62 18·1
6.	S Cassiopeiæ	1 11 34	+72 1·9	58.	R Leo. Min.	9 38 59	+35 1·2
7.	S Piscium	1 11 49	+ 8 21·1	59.	R Leonis	9 41 39	+11 56·4
8.	U Piscium	1 17 9	+12 17·4	60.	*l* Carini	9 42 14	−62 0·1
9.	R Sculptoris	1 21 55	−33 6·8	61.	V Leonis	9 53 57	+21 47·2
10.	R Piscium	1 24 58	+ 2 18·8	62.	U Hydræ	10 32 7	−12 48·8
11.	S Arietis	1 58 43	+11 59·9	63.	R Ursæ Maj.	10 36 50	+69 21·2
12.	R Arietis	2 9 51	+24 32·7	64.	W Leonis	10 47 49	+14 18·1
13.	ο Ceti	2 13 47	− 3 28·6	65.	R Crateris	10 55 9	−17 44·0
14.	S Persei	2 14 59	+58 5·0	66.	S Leonis	11 5 9	+ 6 3·5
15.	R Ceti	2 20 24	− 0 40·5	67.	R Comæ	11 58 37	+19 23·9
16.	U Ceti	2 28 26	−13 37·8	68.	T Virginis	12 8 58	− 5 25·4
17.	T Arietis	2 42 11	+17 3·1	69.	R Corvi	12 13 56	−18 38·6
18.	Algol	3 1 1	+40 32·0	70.	Y Virginis	12 28 12	− 3 48·9
19.	R Persei	3 23 3	+35 17·7	71.	T Ursæ Maj.	12 31 23	+60 5·6
20.	λ Tauri	3 54 35	+12 10·8	72.	R Virginis	12 32 55	+ 7 35·6
21.	W Tauri	4 21 45	+15 51·3	73.	S Ursæ Maj.	12 39 7	+61 41·8
22.	R Tauri	4 22 16	+ 9 55·0	74.	U Virginis	12 45 31	+ 6 9·2
23	S Tauri	4 23 10	+ 9 42·1	75.	W Virginis	13 20 21	− 2 48·4
24.	R Reticuli	4 32 24	−63 15·4	76.	V Virginis	13 22 7	− 2 36·1
25.	V Tauri	4 45 39	+17 21·1	77.	R Hydræ	13 23 42	−22 42·7
26.	R Orionis	4 53 2	+ 7 57·8	78.	S Virginis	13 27 15	− 6 37·7
27.	R Leporis	4 54 35	−14 58·3	79.	W Hydræ	13 42 50	−27 49·1
28.	R Aurigæ	5 8 24	+53 27·7	80.	R² Virginis	13 59 3	− 8 40·2
29.	S Orionis	5 23 35	− 4 46·9	81.	Z Virginis	14 4 25	−12 47·0
30.	U Orionis	5 49 17	+20 9·3	82.	X Boötis	14 18 59	+16 49·3
31.	η Geminorum	6 8 14	+22 32·3	83.	S Boötis	14 19 12	+54 18·7
32.	V Monocerotis	6 17 11	− 2 8·5	84.	V Boötis	14 25 20	+39 21·0
33.	T Monocerotis	6 19 17	+ 7 8·8	85.	R Camelopardi	14 25 55	+84 19·9
34.	R Lyncis	6 52 13	+55 29·1	86.	R Boötis	14 32 20	+27 12·8
35.	ζ Geminorum	6 57 35	+20 43·8	87.	U Boötis	14 49 15	+18 8·6
36.	R Geminorum	7 0 43	+22 52·4	88.	δ Libræ	14 55 6	− 8 4·9
37.	R Canis Min.	7 2 39	+10 11·8	89.	U Coronæ	15 13 43	+32 2·9
38.	L² Puppis	7 10 12	−44 27·8	90.	S Libræ	15 15 4	−19 59·5
39.	R Canis Maj.	7 14 30	−16 11·2	91.	S Serpentis	15 16 31	+14 42·5
40.	V Geminorum	7 16 59	+13 18·1	92.	S Coronæ	15 16 55	+31 45·7
41.	U Monocerotis	7 25 32	− 9 32·8	93.	X Libræ	15 29 52	−20 47·9
42.	S Canis Min.	7 26 45	+ 8 33·2	94.	W Libræ	15 31 42	−15 48·8
43.	T Canis Min.	7 27 53	+11 58·8	95.	U Libræ	15 35 39	−20 49·5
44.	U Canis Min.	7 35 22	+ 8 38·2	96.	Z Libræ	15 40 7	−20 47·2
45.	S Geminorum	7 36 26	+23 42·6	97.	V Coronæ	15 45 37	+39 54·1
46.	T Geminorum	7 42 41	+24 0·5	98.	R Serpentis	15 45 38	+15 28·1
47.	U Geminorum	7 48 34	+22 17·5	99.	R² Libræ	15 50 5	−17 58·8
48.	U Puppis	7 55 42	−12 33·7	100.	Z Scorpii	15 59 36	−21 25·9
49.	R Cancri	8 10 30	+12 3·9	101.	R Herculis	16 1 17	+18 40·1
50.	V Cancri	8 15 26	+17 38·0	102.	X Scorpii	16 2 5	−21 13·8
51.	U Cancri	8 29 28	+19 16·5	103.	W Scorpii	16 5 20	−19 51·0
52.	S Cancri	8 37 39	+19 25·8	104.	R Scorpii	16 11 5	−22 40·3

Mean Places of Variable Stars for 1890 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
105.	S Scorpii.........	16 11 6	−22 37·2	138.	S Cygni	20 3 11	+57 40·1
106.	W Ophiuchi ...	16 15 29	− 7 26·2	139.	R Capricorni ...	20 5 8	−14 35·7
107.	V Ophiuchi ...	16 20 37	−12 10·0	140.	S Aquilæ.........	20 6 34	+15 17·7
108.	U Herculis......	16 20 57	+19 8·4	141.	W Capricorni...	20 8 0	−22 18·6
109.	Y Scorpii	16 23 17	−19 16·2	142.	R Sagittæ	20 9 3	+16 23·6
110.	T Ophiuchi......	16 27 26	−15 53·8	143.	R Delphini......	20 9 37	+ 8 45·4
111.	S Ophiuchi......	16 27 55	−16 55·7	144.	U Cygni	20 16 12	+47 32·8
112.	W Herculis......	16 31 20	+37 33·9	145.	V Cygni	20 37 45	+47 44·9
113.	R Ursæ Min. ...	16 31 27	+72 30·0	146.	S Delphini	20 38 1	+16 41·6
114.	R Draconis ...	16 32 22	+66 59·2	147.	X Cygni	20 39 5	+35 11·4
115.	S Herculis	16 46 53	+15 7·6	148.	T Delphini	20 40 16	+15 59·9
116.	R Ophiuchi......	17 1 26	−15 56·6	149.	U Capricorni ...	20 42 1	−15 11·3
117.	U Ophiuchi......	17 10 56	+ 1 20·0	150.	T Aquarii	20 44 8	− 5 33·3
118.	X Sagittarii ...	17 40 37	−27 47·2	151.	T Vulpeculæ ...	20 46 48	+27 50·1
119.	Y Ophiuchi......	17 46 45	− 6 6·9	152.	Y Cygni	20 47 40	+34 14·7
120.	W Sagittarii ...	17 58 0	−29 35·1	153.	R Vulpeculæ ...	20 59 30	+23 23·2
121.	T Herculis	18 4 56	+31 0·1	154.	V Capricorni ...	21 1 12	−24 21·7
122.	Y Sagittarii......	18 14 55	−18 54·5	155.	X Capricorni ...	21 2 15	−21 47·5
123.	T Serpentis......	18 23 27	+ 6 13·5	156.	T Cephei.........	21 8 4	+68 2·6
124.	U Sagittarii ...	18 25 25	−19 12·3	157.	T Capricorni ...	21 15 57	−15 37·4
125.	X Ophiuchi ...	18 33 7	+ 8 43·9	158.	Y Capricorni ...	21 28 23	−14 27·6
126.	R Scuti	18 41 37	− 5 49·5	159.	W Cygni.........	21 31 53	+44 53·0
127.	β Lyræ	18 46 1	+33 14·1	160.	S Cephei.........	21 36 35	+78 7·7
128.	R Lyræ	18 51 59	+43 48·0	161.	U Aquarii	21 57 20	−17 9·4
129.	R Aquilæ	19 1 4	+ 8 3·8	162.	T Pegasi.........	22 3 31	+12 0·1
130.	T Sagittarii ...	19 9 53	−17 9·8	163.	δ Cephei	22 25 5	+57 51·1
131.	R Sagittarii ...	19 10 15	−19 30·2	164.	R Lacertæ	22 38 23	+41 47·7
132.	S Sagittarii......	19 12 59	−19 13·5	165.	S Aquarii	22 51 12	−20 55·7
133.	U Aquilæ	19 23 26	− 7 16·2	166.	S Pegasi	23 14 59	+ 8 19·0
134.	S Vulpeculæ ...	19 43 53	+27 0·7	167.	R Aquarii	23 38 8	−15 53·7
135.	χ Cygni	19 46 20	+32 38·1	168.	V Ceti	23 52 16	− 9 34·4
136.	η Aquilæ.........	19 46 52	+ 0 43·4	169.	R Cassiopeiæ ...	23 52 48	+50 46·5
137.	S Sagittæ	19 51 1	+16 20·5				

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's Name refers to Catalogue on pp. 23–24.)

Note.—The times of minima only are given; the times of maxima may be found by adding the inte printed under the name of the star.

T Monocer. 33.

(7^d 22^h.)

Jan.		May		Oct.	
d	h	d	h	d	h
	4	14	5	33	5
Feb.					
	4	Aug.		Nov.	
		3	5	19	5
Mar.		30	5		
	4			Dec.	
Apr.		Sept.		16	5
	5	26	5		

Geminor. 35.

(5^d 9^h.)

Jan.		Apr.		Sept.	
d	h	d	h	d	h
	1	19	14	29	1
	4	29	17		
	8			Oct.	
Feb.		May		9	4
		9	21	19	8
	12	20	1	29	12
	16	30	4		
	19			Nov.	
Mar.		Aug.		8	16
				18	19
	23	19	10	28	23
	3	29	14		
	6			Dec.	
		Sept.			
Apr.				9	3
		8	17	19	6
	10	18	21	29	10

Virginis 75.

(8^d 5^h.)

Jan.		Feb.		Mar.	
	h	d	h	d	h
	15	2	21	9	10
		20	4	26	17

W Virginis 75.

(*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
12	23	8	8	19	23
30	6	25	14		
				Nov.	
May		Aug.		6	5
17	12	11	21	23	12
		29	3		
June				Dec.	
		Oct.			
3	19			10	18
21	1	2	16	28	1

X Sagittar. 118.

(2^d 21^h.)

Feb.		May		Aug.	
d	h	d	h	d	h
6	13	15	17	21	21
13	13	22	17	28	21
20	14	29	18		
27	14			Sept.	
		June		4	22
Mar.		5	18	11	22
6	14	12	18	18	22
13	15	19	19	25	23
20	15	26	19		
27	15			Oct.	
		July		2	23
Apr.				9	23
		3	19	16	23
3	15	10	19	24	0
10	16	17	20	31	0
17	16	24	20		
24	16	31	20		
				Nov.	
May		Aug.		7	0
				14	1
1	17	7	21	21	1
8	17	14	21	28	1

Y Ophiuchi 119.

(7^h 21^h.)

Feb.		May		Aug.	
d	h	d	h	d	h
6	1	19	22	30	18
23	5				
		June		Sept.	
Mar.		6	1	16	21
12	8	23	4		
29	11			Oct.	
		July		4	0
Apr.		10	8	21	4
15	15	27	11		
May		Aug.		Nov.	
2	18	13	14	7	7

W Sagittar. 120.

(3^d 0^h.)

Feb.		May		Aug.	
d	h	d	h	d	h
13	17	23	10	30	4
21	7	31	1		
28	21			Sept.	
		June		6	18
Mar.		7	15	14	9
8	12	15	5	21	23
16	2	22	20	29	13
23	16	30	10		
31	7			Oct.	
		July		7	3
Apr.		8	0	14	18
7	21	15	14	22	8
15	11	23	5	29	22
23	1	30	19		
30	16			Nov.	
		Aug.		6	12
May		7	9	14	3
8	6	14	23	21	17
15	20	22	14	29	7

Y Sagittar. 122.

(1^d 19^h.)

Feb.		May		Aug.	
d	h	d	h	d	h
1	18	16	14	28	11
7	13	22	9		
13	7	28	3	Sept.	
19	2				
24	20	June		3	5
				9	0
Mar.		2	22	14	18
		8	16	20	12
2	15	14	11	26	7
8	9	20	5		
14	3	26	0	Oct.	
19	22				
25	16	July		2	1
31	11	1	18	7	20
		7	13	13	14
Apr.		13	7	19	9
6	5	19	1	25	3
12	0	24	20	30	22
17	18	30	14		
23	13			Nov.	
29	7	Aug.		5	16
		5	9	11	11
May		11	3	17	5
5	2	16	22	23	0
10	20	22	16	28	18

U Sagittar. 124.

(2^d 23^h.)

Feb.		Apr.		June	
d	h	d	h	d	h
5	9	7	2	6	18
12	3	13	19	13	12
18	20	20	13	20	6
25	14	27	7	27	0
Mar.		May		July	
4	8	4	1	3	18
11	2	10	19	10	12
17	20	17	13	17	6
24	14	24	7	24	0
31	8	31	1	30	18

U Sagittar.

(*continued*)

Aug.		Sept.	
d	h	d	h
6	11	15	23
13	5	22	16
19	23	29	10
26	17		
		Oct.	
Sept.		6	4
2	11	12	22
9	5	19	16

β Lyræ

(Max. 3^d 5

Secondary

6^d 11^h.

Max. 9^d 16^h

Feb.		June	
d	h	d	h
9	12	5	18
22	10	18	16
Mar.		July	
7	8	1	14
20	6	14	12
		27	10
Apr.		Aug.	
2	4	9	8
15	2	22	6
28	0		
May		Sept.	
10	22	4	4
23	20	17	2

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 133.

(2d 12h.)

Feb.		May		Sept.	
d	h	d	h	d	h
4	1	23	12	6	0
1	2	30	13	13	1
8	3	June		20	2
Mar.		6	14	27	3
7	4	13	15	Oct.	
4	5	20	16	4	4
1	5	27	16	11	4
8	6	July		18	5
Apr.		4	17	25	6
4	7	11	18	Nov.	
1	8	18	19	1	7
8	8	25	20	8	7
5	9	Aug.		15	8
May		1	20	22	9
2	10	8	21	29	10
9	11	15	22	Dec.	
6	12	22	23	6	11
		30	0	13	11

η AQUILÆ 136.

(2d 9h.)

Feb.		Mar.		Mar.	
d	h	d	h	d	h
8	5	4	13	18	22
5	9	11	17	26	2

η AQUILÆ 136 (*continued*).

Apr.		June		Sept.	
d	h	d	h	d	h
2	6	27	9	21	12
9	10	July		28	16
16	15	4	13	Oct.	
23	19	11	17	5	20
30	23	18	22	13	0
May		26	2	20	5
8	3	Aug.		27	9
15	8	2	6	Nov.	
22	12	9	10	3	13
29	16	16	15	10	17
June		23	19	17	22
5	20	30	23	25	2
13	1	Sept.		Dec.	
20	5	7	3	2	6
		14	8	9	10
				16	15

S SAGITTÆ 137.

(3d 0h.)

Feb.		Mar.		Apr.	
d	h	d	h	d	h
16	6	13	10	7	13
24	16	21	19	15	23
Mar.		30	4	24	8
5	1				

S SAGITTÆ 137 (*continued*).

May		July		Oct.	
d	h	d	h	d	h
2	17	25	13	8	23
11	2	Aug.		17	9
19	11	2	22	25	18
27	21	11	7	Nov.	
June		19	16	3	3
5	6	28	2	11	12
13	15	Sept.		19	21
22	0	5	11	28	7
30	9	13	20	Dec.	
July		22	5	6	16
8	18	30	14	15	1
17	4				

X CYGNI 147.

(6d 21½h.)

Mar.		July		Oct.	
d	h	d	h	d	h
14	10	7	3	13	10
30	19	23	12	29	20
Apr.		Aug.		Nov.	
16	5	8	21	15	5
May		25	7	Dec.	
2	14	Sept.		1	14
18	23	10	16	17	23
June		27	1		
4	8				
20	18				

T VULPEC. 151.

(1d 1½h.)

Mar.		June		Sept.	
d	h	d	h	d	h
3	13	4	18	19	5
8	0	9	4	23	16
12	10	13	15	28	2
16	21	18	1	Oct.	
21	7	22	12	2	13
25	18	26	22	6	23
30	4	July		11	10
Apr.		1	9	15	20
3	15	5	19	20	7
8	1	10	5	24	17
12	12	14	16	29	4
16	22	19	2	Nov.	
21	9	23	13	2	14
25	19	27	23	7	1
30	6	Aug.		11	11
May		1	10	15	21
4	16	5	20	20	8
9	3	10	7	24	18
13	13	14	17	29	5
18	0	19	4	Dec.	
22	10	23	14	3	15
26	21	28	1	8	2
31	7	Sept.		12	12
		1	11	16	23
		5	22	21	9
		10	8	25	20
		14	19	30	6

δ CEPHEI 1

(1d 15h.)

Jan.		May	
d	h	d	h
4	19	2	20
10	4	8	5
15	13	13	14
20	21	18	23
26	6	24	8
31	15	29	16
Feb.		June	
6	0	4	1
11	9	9	10
16	17	14	19
22	2	20	4
27	11	25	12
Mar.		30	21
4	20	July	
10	4	6	6
15	13	11	15
20	22	17	0
26	7	22	8
31	16	27	17
Apr.		Aug.	
6	0	2	2
11	9	7	11
16	18	12	20
22	3	18	4
27	12	23	13
		28	22

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1879 …………	18 41 west.	0·1803	67 37 observed.
1889 …………	17 35 „	0·1821	67 24 „
1891 …………	17 24 „	0·1825	67 21 inferred.

The horizontal force is given in C. G. S. measure.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following star's name are the Nos. in Catalogue, pp. 23–24.)

U CEPHEI 5 *.

($2^d\ 11^h\ 50^m$.)

Jan. d	h	m
1	8	49
6	8	28
11	8	8
16	7	48
21	7	27
26	7	7
31	6	47

Feb. d	h	m
5	6	26
10	6	6
15	5	46
20	5	26
25	5	5

March. d	h	m
2	4	45
7	4	25
12	4	4
17	3	44
22	3	14
27	3	3

April. d	h	m
1	2	43
6	2	23
11	2	3
16	1	42
21	1	22
26	1	2

May. d	h	m
1	0	41
6	0	21
11	0	1
15	23	40
20	23	20
25	23	0
30	22	40

June. d	h	m
4	22	19
9	21	59
14	21	39
19	21	18
24	20	58
29	20	38

July. d	h	m
4	20	17
9	19	57
14	19	37
19	19	17
24	18	56
29	18	36

August. d	h	m
3	18	16
8	17	55
13	17	35
18	17	15
23	16	55
28	16	34

Sept. d	h	m
2	16	14
7	15	54
12	15	33
17	15	13
22	14	53
27	14	33

Oct. d	h	m
2	14	12
7	13	52
12	13	32
17	13	11
22	12	51
27	12	31

Nov. d	h	m
1	12	11
6	11	50
11	11	30
16	11	10
21	10	50
26	10	29

Dec. d	h	m
1	10	9
6	9	49
11	9	28
16	9	8
21	8	48
26	8	28
31	8	7

ALGOL 18 *.

($2^d\ 20^h\ 49^m$.)

Jan. d	h	m
2	13	11
8	6	49
14	0	26
19	18	14
25	11	42
31	5	20

Feb. d	h	m
5	22	57
11	16	35
17	10	13
23	3	50
28	21	28

March. d	h	m
6	15	6
12	8	43
18	2	21
23	19	59
29	13	37

April. d	h	m
4	7	14
10	0	52
15	18	30
21	12	7
27	5	45

July. d	h	m
2	4	29
7	22	6
13	15	44
19	9	22
25	2	59
30	20	37

August. d	h	m
5	14	15
11	7	52
17	1	30
22	19	8
28	12	46

Sept. d	h	m
3	6	23
9	0	1
14	17	38
20	11	16
26	4	54

Oct. d	h	m
1	22	32
7	16	9
13	9	47
19	3	25
24	21	3
30	14	40

Nov. d	h	m
5	8	18
11	1	56
16	19	33
22	13	11
28	6	49

Dec. d	h	m
4	0	26
9	18	4
15	11	42
21	5	20
26	22	57

λ TAURI 20 *.

($3^d\ 22^h\ 52^m$.)

Jan. d	h	m
2	3	2
10	0	47
17	22	31
25	20	15

Feb. d	h	m
2	18	0
10	15	44
18	13	29
26	11	13

March. d	h	m
6	8	57
14	6	42
22	4	26
30	2	11

July. d	h	m
2	23	3
10	20	48
18	18	32
26	16	17

λ TAURI 20 *

(*continued*).

August. d	h	m
3	14	1
11	11	45
19	9	30
27	7	14

Sept. d	h	m
4	4	59
12	2	43
20	0	27
27	22	12

Oct. d	h	m
5	19	56
13	17	41
21	15	25
29	13	9

Nov. d	h	m
6	10	54
14	8	38
22	6	23
30	4	7

Dec. d	h	m
8	1	51
15	23	36
23	21	20
31	19	5

R CANIS MAJ. 39 †.

$\left\{\begin{array}{l} 1 = 1^d\ 3^h\ 16^m. \\ 2 = 2\ \ 6\ \ 31 \end{array}\right\}$

Jan. d	h	m
1	9	47
4	19	35
8	5	22
11	15	9
15	0	57
18	10	44
21	20	31
25	6	19
28	16	6

Feb. d	h	m
1	1	53
4	11	41
7	21	28
11	7	15
14	17	2
18	2	50
21	12	37
24	22	24
28	8	12

March. d	h	m
3	17	59
7	3	46
10	13	34
13	23	21
17	9	9
20	18	55
24	4	43
27	14	30
31	0	17

April. d	h	m
3	10	5
6	19	52
10	5	39
13	15	27
17	1	14
20	11	1
23	20	48
27	6	36
30	16	23

May. d	h	m
4	2	10
7	11	58
10	21	45
14	7	32
17	17	20
21	3	7
24	12	54

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* Alternate minima *only are given*; the others can be readily found by

val under the name of the star.

inimum only.

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 39† (*continued*).

Sept.			Nov.		
d	h	m	d	h	m
1	12	2	1	20	13
4	21	49	5	6	0
8	7	36	8	15	48
11	17	24	12	1	35
15	3	11	15	11	22
18	12	58	18	21	10
21	22	45	22	6	57
25	8	33	25	16	44
28	18	20	29	2	31

Oct.			Dec.		
2	4	7	2	12	19
5	13	55	5	22	6
8	23	42	9	7	53
12	9	29	12	17	41
15	19	17	16	3	28
19	5	4	19	13	15
22	14	51	22	23	3
26	0	38	26	8	50
29	10	26	29	18	37

S Cancri 52.

Jan.			June.		
d	h	m	d	h	m
4	4	40	4	22	44
13	16	18	14	10	22
23	3	56	23	22	0

Feb.			Sept.		
1	15	34	7	19	2
11	3	11	17	6	40
20	14	49	26	18	17

March.			Oct.		
2	2	27	6	5	55
11	14	5	15	17	33
21	1	42	25	5	11
30	13	20			

April.			Nov.		
9	0	58	3	16	48
18	12	36	13	4	26
28	0	13	22	16	4

May.			Dec.		
7	11	51	2	3	42
16	23	29	11	15	19
26	11	7	21	2	57
			30	14	35

δ Libræ 88 *.

$(2^d\ 7^h\ 51^m.)$

Jan.			June.		
d	h	m	d	h	m
1	14	30	4	5	1
6	6	12	8	20	44
10	21	55	13	12	26
15	13	38	18	4	9
20	5	21	22	19	52
24	21	3	27	11	35
29	12	46			

Feb.			July.		
3	4	29	2	3	17
7	20	12	6	19	0
12	11	55	11	10	43
17	3	37	16	2	26
21	19	20	20	18	8
26	11	3	25	9	51
			30	1	34

March.			August.		
3	2	46	3	17	17
7	18	28	8	8	59
12	10	11	13	0	42
17	1	54	17	16	25
21	17	37	22	8	8
26	9	19	26	23	50
31	1	2	31	15	33

April.			Sept.		
4	16	45	5	7	16
9	8	28	9	22	59
14	0	10	14	14	41
18	15	53	19	6	24
23	7	36	23	22	7
27	23	19	28	13	50

May.			Dec.		
2	15	1	2	17	48
7	6	44	7	9	31
11	22	27	12	1	14
16	14	10	16	16	57
21	5	52	21	8	39
25	21	35	26	0	22
30	13	18	30	16	5

U Coronæ 89 *.

$(3^d\ 10^h\ 51^m.)$

Jan.			Feb.		
d	h	m	d	h	m
2	8	6	5	20	37
9	5	48	12	18	19
16	3	30	19	16	1
23	1	13	26	13	43
29	22	55			

U Coronæ 89 * (*continued*).

March.			July.		
d	h	m	d	h	m
5	11	25	7	18	4
12	9	8	14	15	46
19	6	50	21	13	28
26	4	32	28	11	10

April.			August.		
2	2	14	4	8	53
8	23	56	11	6	35
15	21	38	18	4	17
22	19	20	25	1	59
29	17	3	31	23	41

May.			Sept.		
6	14	45	7	21	23
13	12	27	14	19	5
20	10	9	21	16	48
27	7	51	28	14	30

June.			Dec.		
3	5	33	6	15	31
10	3	15	13	13	13
17	0	58	20	10	55
23	22	40	27	8	38
30	20	22			

U Ophiuchi 117‡.

$$\left\{\begin{array}{llll} 1 = 0^d & 20^h & 8^m \\ 2 = 1 & 16 & 16 \\ 3 = 2 & 12 & 22 \end{array}\right\}$$

Feb.			April.		
d	h	m	d	h	m
1	3	30	2	12	42
4	12	0	5	21	12
7	20	31	9	5	43
11	5	2	12	14	14
14	13	32	15	22	44
17	22	3	19	7	15
21	6	34	22	15	46
24	15	4	26	0	16
27	23	35	29	8	47

March.			May.		
3	8	6	2	17	17
6	16	36	6	1	48
10	1	7	9	10	19
13	9	38	12	18	50
16	18	8	16	3	20
20	2	39	19	11	51
23	11	10	22	20	22
26	19	40	26	4	52
30	4	11	29	13	23

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* Alternate minima only are given; the others can be readily found by *adding* the interval under the name of the star.

† *Every third* minimum only.

‡ *Every fourth* minimum only.

Minima of Variable Stars of the Algol Type (continued).

U OPHIUCHI 117 ‡ (*continued*).

June.			July.			August.			Sept.			Oct.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
1	21	54	2	2	29	1	7	5	3	20	12	4	0	48
5	6	24	5	11	0	4	15	36	7	4	43	7	9	19
8	14	55	8	19	31	8	0	7	10	13	13	10	17	49
11	23	26	12	4	1	11	8	37	13	21	44	14	2	20
15	7	56	15	12	32	14	17	8	17	6	15	17	10	50
18	16	27	18	21	3	18	1	39	20	14	45	20	19	21
22	0	58	22	5	33	21	10	9	23	23	16	24	3	52
25	9	28	25	14	4	24	18	40	27	7	46	27	12	22
28	17	59	28	22	35	28	3	11	30	16	17	30	20	53
						31	11	41						

Y CYGNI 152 ‡.

$$\left\{\begin{array}{l} 1 = 1^d\ 11^h\ 57^m \\ 2 = 2\ \ 23\ \ 55 \\ 3 = 4\ \ 11\ \ 52 \end{array}\right\}$$

Jan.			Feb.			March.			April.			May.			June.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
3	14	36	2	13	42	4	12	48	3	11	54	3	11	0	2	10	6
9	14	26	8	13	32	10	12	38	9	11	44	9	10	50	8	9	56
15	14	15	14	13	21	16	12	27	15	11	33	15	10	39	14	9	45
21	14	4	20	13	10	22	12	16	21	11	22	21	10	28	20	9	34
27	13	53	26	12	59	28	12	5	27	11	11	27	10	17	26	9	23

Y CYGNI 152 ‡ (*continued*).

July.			August.			Sept.			Oct.			Nov.			Dec.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
2	9	12	1	8	18	6	7	14	6	6	20	5	5	26	5	4	32
8	9	2	7	8	8	12	7	3	12	6	9	11	5	15	11	4	21
14	8	51	13	7	57	18	6	52	18	5	58	17	5	4	17	4	10
20	8	40	19	7	46	24	6	41	24	5	47	23	4	53	23	3	59
26	8	29	25	7	35	30	6	30	30	5	36	29	4	42	29	3	48
			31	7	24												

S ANTLIÆ 56 §.

Jan.			Feb.			March.			April.			May.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
0	11	44	1	21	44	3	1	56	1	6	8	3	16	8
3	17	32	5	3	32	6	7	44	4	11	56	6	21	56
6	23	20	8	9	20	9	13	32	7	17	44	10	3	44
10	5	8	11	15	8	12	19	20	10	23	32	13	9	32
13	10	56	14	20	56	16	1	8	14	5	20	16	15	20
16	16	44	18	2	44	19	6	56	17	11	8	19	21	8
19	22	32	21	8	32	22	12	44	20	16	56	23	2	56
23	4	20	24	14	20	25	18	32	23	22	44	26	8	44
26	10	8	27	20	8	29	0	20	27	4	32	29	14	32
29	15	56							30	10	20			

S ANTLIÆ 56 § (*continued*).

June.			Oct.			Nov.			Dec.		
d	h	m	d	h	m	d	h	m	d	h	m
1	20	20	3	0	44	1	4	56	3	14	56
5	2	8	6	6	32	4	10	44	6	20	44
8	7	56	9	12	20	7	16	32	10	2	32
11	13	44	12	18	8	10	22	20	13	8	20
14	19	32	15	23	56	14	4	8	16	14	8
18	1	20	19	5	44	17	9	56	19	19	56
21	7	8	22	11	32	20	15	44	23	1	44
24	12	56	25	17	20	23	21	32	26	7	32
27	18	44	28	23	8	27	3	20	29	13	20
						30	9	8			

‡ Every fourth minimum only.

§ Every tenth minimum only, the others may be found by adding the multiples of period in the following table:—

P.	h m	P.	d h m	P.	d h m
1	7 47	4	1 7 7	7	2 6 28
2	15 34	5	1 14 54	8	2 14 14
3	23 20	6	1 22 41	9	2 22 1

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to catalogue on pp. 23–24; M, signifies maximum; *m*, minimum.

January.

d	No.	
2	27	*m*
	166	M
4	102	M
5	24	M
	30	M
7	60	M
	70	*m*
8	66	M
	23	M
	115	*m*
	126	*m*
	134	*m*
9	128	*m*
10	69	M
12	90	*m*
13	72	M
14	144	*m*
	62	*m*
15	82	*m*
	95	M
	76	M
18	38	*m*
	60	*m*
20	148	M
21	85	M
23	55	*m*
24	40	*m*
	153	*m*
	128	M
26	41	*m*
	9	M
27	142	*m*
29	134	M
31	146	M

February.

d	No.	
1	71	M
4	13	*m*
7	138	M
	60	M
9	19	M
	37	*m*
12	44	M
	105	M
	103	M
	126	M
	135	*m*
13	41	M
	114	*m*
17	161	M
18	60	*m*
20	29	*m*
	107	M
23	8	*m*
24	128	*m*
25	47	M
26	65	M
28	159	M
	108	M

March.

d	No.	
2	21	M
	142	*m*
3	78	M
	81	M
6	87	M
	50	*m*
7	4	M
	1	*m*
9	36	*m*
10	60	M
11	128	M
12	41	*m*
13	74	*m*
17	134	*m*
	157	M
18	121	M
20	126	*m*
21	60	*m*
	12	M
	11	*m*
22	57	M
23	15	*m*
25	79	M
	109	M
26	86	*m*
29	84	M
30	38	M
	72	*m*
	41	M
31	80	M
	153	M

April.

d	No.	
1	113	*m*
4	11	M
5	92	M
6	96	M
	25	M
	134	M
7	73	*m*
	142	*m*
9	28	*m*
	82	M
	70	M
10	60	M
11	91	M
	63	M
	139	M
	128	*m*
	62	M
7	106	M
9	129	*m*
	132	M
	154	M
20	112	M
21	60	*m*
22	143	M
	90	M
24	160	*m*
	126	M
25	165	M
26	128	M
	32	M
	162	M
	41	*m*
27	167	M
	158	M

May.

d	No.	
3	159	*m*
5	9	*m*
8	123	M
9	34	M
11	60	M
12	142	*m*
3	140	*m*
4	41	M
15	48	M
16	141	M
17	94	M
18	150	M
	82	*m*
	164	M
22	60	*m*
23	47	M
	59	*m*
24	134	*m*
	113	M
	8	M
25	13	M
	65	*m*
26	10	M
27	128	*m*
28	87	*m*
30	126	*m*
31	100	M

June.

d	No.	
1	77	M
	42	M
	114	M
2	111	M
3	38	*m*
	15	M
	37	M
5	54	M
6	86	M
7	72	M
9	40	M
	74	M
10	41	*m*
11	60	M
	128	M
12	153	*m*
13	121	*m*
	134	M
14	115	M
15	12	*m*
	50	M
16	142	*m*
22	60	*m*
	116	M
24	155	M
	156	M
26	71	*m*
	17	*m*
27	61	M
28	41	M
30	43	M

July.

d	No.	
3	104	M
4	159	M
5	126	M
8	3	M
9	93	M
2	60	M
	64	M
	125	M
	99	M
	128	*m*
8	66	M
21	142	*m*
22	53	M
	102	M
23	60	*m*
	146	*m*
24	90	*m*
	73	M
27	128	M
28	62	*m*
30	26	M
31	134	*m*
	97	M

August.

d	No.	
2	7	M
3	135	M
4	31	*m*
5	70	*m*
	65	M
8	105	M
10	126	*m*
	82	M
12	60	M
13	41	M
	38	M
14	45	M
15	8	*m*
	83	M
16	60	*m*
17	153	M
	47	M
20	67	M
	134	M
21	9	M
22	27	M
23	60	*m*
	72	*m*
24	84	*m*
25	6	M
26	142	*m*
27	87	M
	128	*m*
29	44	*m*
	168	M
30	121	M
	4	M
31	101	M

September.

d	No.	
1	58	M
	144	M
	131	M
	21	*m*
2	95	M
3	129	M
6	159	*m*
	15	*m*
7	19	M
8	29	M
9	41	*m*
11	22	M
	128	M
12	60	M
14	126	M
	150	*m*
18	82	*m*
19	112	*m*
22	25	M
23	76	M
	60	*m*
24	12	M
	154	M
25	103	M
	51	M
27	41	M
	169	*m*
28	113	*m*
30	142	*m*

October.

d	No.	
6	74	*m*
	134	*m*
7	140	*m*
12	128	*m*
13	60	M
	24	M
17	71	M
	38	*m*
	114	*m*
18	59	M
	85	M
20	126	*m*
21	108	*m*
22	68	M
23	62	M
24	60	*m*
	63	*m*
	41	*m*
25	2	M
27	134	M
	17	M
	128	M
	40	*m*
28	153	*m*
30	72	M

November.

d	No.	
1	90	M
	65	*m*
2	161	M
3	80	M
	55	M
	145	M
4	146	M
	142	*m*
5	70	M
	86	*m*
7	49	M
	159	M
8	1	M
11	41	M
	47	M
12	42	*m*
13	60	M
14	8	M
16	166	M
17	15	M
18	87	*m*
19	73	*m*
	158	M
20	113	M
21	28	M
22	115	*m*
	110	M
	4	M
23	69	M
24	126	M
	60	*m*
25	121	*m*
27	128	*m*
28	9	*m*
30	14	M

December.

d	No.	
1	92	*m*
3	50	*m*
4	130	M
5	132	M
7	78	*m*
	150	M
8	41	*m*
9	94	M
	142	*m*
11	82	M
	141	M
12	157	M
	128	M
13	134	*m*
14	60	M
16	34	*m*
18	16	M
	148	M
19	12	*m*
	83	*m*
21	84	M
22	160	M
24	107	M
25	98	M
	60	*m*
26	138	M
	41	M
27	156	*m*
	38	M
29	13	*m*
30	126	*m*
31	81	M

DOUBLE STARS.

Name of Star.	R.A. 1890.	Dec. 1890.	Mag.	Position.	Distance.	Epoch. 1880+.	Observer.
	h m	° ′		°	″		
Σ 13, Cephei 318	0 10·0	+76 20	7 ; 7	88·8	0·60	7·77	T.
OΣ 4	0 10·5	+35 52	7 ; 8	141·1	0·30	7·93	HΣ.
OΣ 12, λ Cassiop.	0 25·7	+53 55	6½ ; 6½	144·2	0·45	7·52	T.
Σ 60, η Cassiop.	0 42·5	+57 14	3½ ; 7½	184·7	4·76	8·26	M.
OΣ 20, 66 Piscium ..	0 48·8	+18 36	6 ; 7	350·7	0·42	8·78	HΣ.
Σ 73, 36 Andromedæ .	0 48·9	+23 2	6 ; 6½	6·1	1·21	8·51	L.
Σ 113, 42 Ceti	1 14·0	− 1 5	6½ ; 7	349·6	1·51	8·44	L.
Σ 138	1 30·3	+ 7 5	7 ; 7	213·9	1·60	8·55	L.
p Eridani	1 35·7	−56 45	6 ; 6	228·1	6·66	8·04	Te.
Σ 186	1 51·0	+ 1 20	7 ; 7	206·3	0·28	8·05	S.
Σ 208, 10 Arietis	1 57·4	+25 24	6 ; 8	49·2	0·93	8·05	S.
Σ 228	2 6·9	+46 58	6½ ; 7½	30·8	0·35	9·02	S.
Hastings	2 11·0	−18 44	8 ; 9	339·6	2·18	8·66	L.
Σ 333, ε Arietis.........	2 52·9	+20 54	5½ ; 6	201·8	1·23	8·94	L.
Σ 367	3 8·0	+ 0 20	7½ ; 7½	229·4	0·84	8·06	L.
Σ 412, 7 Tauri	3 27·9	+24 6	6½ ; 7	216·0	0·32	9·03	T.
Σ 422......................	3 32·5	+ 0 16	6 ; 8	244·4	6·20	8·94	T.
OΣ 79, 55 Tauri	4 13·6	+16 16	6½ ; 8	76·4	0·43	7·59	S.
Σ 535	4 17·2	+11 7	7 ; 8	333·8	1·38	8·47	L.
OΣ 98, *i* Orionis	5 1·9	+ 8 21	5½ ; 7	193·9	0·99	9·03	L.
Σ 728	5 24·9	+ 5 52	6 ; 8	179·2	0·38	9·03	β.
Σ 749	5 30·3	+26 51	6½ ; 7	174·1	1·02	7·18	T.
Σ 948, 12 Lyncis AB .	6 36·5	+59 33	6 ; 6½	124·9	1·57	9·31	M.
Sirius........................	6 40·3	−16 34	1 ; 10	359·7	4·19	10·27	β.
OΣ 159, 15 Lyncis ...	6 47·8	+58 34	5 ; 6	5·9	0·77	10·25	M.
OΣ 170	7 12·1	+ 9 30	7 ; 7½	110·5	1·36	8·16	L.
Σ 1110, Castor AB ...	7 27·0	+32 7	2 ; 3	230·4	5·66	9·25	M.
β 101, 9 Argûs	7 46·7	−13 35	5 ; 7	76·4	0·34	9·08	β.
OΣ 182	7 47·2	+ 3 40	8½ ; 8½	34·2	1·15	8·12	L.
Σ 1157	7 49·0	− 2 31	8 ; 8	247·0	1·28	7·60	T.
Σ 1196, ζ Cancri AB .	8 5·3	+18 0	5 ; 6	35·8	0·91	10·29	C.
β 205	8 28·0	−24 14	7 ; 7	257·0	0·74	9·16	L.
Σ 1273, ε Hydræ	8 41·0	+ 6 49	4 ; 7	228·9	3·21	9·13	L.
Σ 3121	9 11·3	+29 3	7½ ; 7½	286·3	0·22	8·27	S.
Σ 1356, ω Leonis	9 22·6	+ 9 32	5½ ; 7	101·8	0·68	10·27	C.
Σ 1417	10 9·0	+19 42	8½ ; 8½	258·3	2·41	8·83	L.
OΣ 215, Piazzi x. 23...	10 10·3	+18 17	6½ ; 7	213·6	0·74	10·27	C.
Σ 1424, γ Leonis AB .	10 13·9	+20 24	2 ; 4	114·5	3·36	10·27	C.
Σ 1457	10 32·0	+ 6 20	7½ ; 8	313·5	1·18	9·70	M.
OΣ 224	10 34·1	+ 9 26	7 ; 9	315·6	0·53	7·27	S.
Σ 1523, ξ Ursæ Maj. .	11 12·3	+32 9	4 ; 5	209·1	1·96	10·40	M.
Σ 1536, ι Leonis	11 18·2	+11 8	4½ ; 7½	62·0	2·56	9·16	L.
OΣ 234	11 24·8	+41 54	7 ; 7½	324	0·20	6·36	E.
OΣ 235	11 26·1	+61 42	5½ ; 7	72·6	1·32	8·69	T.
OΣ 287	11 47·6	+45 22	7 ; 8	317·5	0·64	7·56	S.
Σ 1639, 68 Comæ......	12 19·0	+26 12	6½ ; 8	236·0	0·22	7·39	S.
Σ 1670, γ Virginis ...	12 36·1	− 0 51	3 ; 3	153·9	5·45	9·29	L.
Σ 1687, 35 Comæ......	12 47·9	+21 51	5 ; 8	69·5	1·30	7·34	T.
Σ 1728, 42 Comæ......	13 4·7	+18 7	5½ ; 6	189·6	0·55	9·09	L.
Σ 1757, Piazzi xiii. 127	13 28·9	+ 0 15	7½ ; 8	72·6	2·50	10·25	[illegible]
Σ 1768, 25 Can. Ven. .	13 33·0	+36 46	6 ; 8	142·7	0·72	7·46	[illegible]
Σ 1788	13 49·2	− 7 31	6½ ; 7½	75·2	2·64	[illegible]	[illegible]
Σ 1819	14 9·8	+ 3 39	7½ ; 7½	10·1	1·33	[illegible]	[illegible]
α Centauri................	14 32·0	−60 33	1 ; 2	202·9	17·12	[illegible]	[illegible]

Double Stars (*continued*).

No.	Name of Star.	R.A. 1890.	Dec. 1890.	Mag.	Position.	Distance.	Epoch 1880+.
		h m	° ′		°	″	
55.	Σ 1865, ζ Boötis	14 36·2	+14 12	4 ; 4	293·4	0·51	7·50
56.	Σ 1876	14 40·8	− 6 55	8 ; 8	251·5	1·30	8·31
57.	Σ 1888, ξ Boötis	14 46·3	+19 33	5 ; 7	246·2	3·15	10·41
58.	β 239	14 52·0	−27 12	6 ; 6	128·3	0·96	8·43
59.	β 119	14 59·7	− 6 35	8 ; 8½	306·4	1·61	8·45
60.	Σ 1937, η Cor. Bor.	15 18·8	+30 42	6 ; 6	186·0	0·72	7·63
61.	Σ 1938, μ^2 Boötis	15 20·4	+37 46	6½ ; 7½	97·8	0·73	9·35
62.	OΣ 298	15 32·5	+40 13	7 ; 7½	143·0	0·33	7·56
63.	Σ 1967, γ Cor. Bor.	15 35·0	+37 0	4 ; 7	126·6	0·38	7·51
64.	OΣ 303	15 56·0	+13 35	7 ; 7½	137·5	0·65	7·47
65.	Σ 1998, ξ Libræ	15 57·7	−11 4	5 ; 5½	200·4	1·24	8·50
66.	Σ 2032, σ Coronæ	16 11·0	+34 12	6 ; 6½	207·8	3·83	8·62
67.	Σ 2055, λ Ophiuchi	16 25·0	+ 2 13	4½ ; 5½	42·0	1·69	8·70
68.	Σ 2084, ζ Herculis	16 37·2	+31 48	3 ; 6½	65·8	1·68	10·70
69.	Σ 2107, 167 Herculis	16 47·6	+28 52	7 ; 8½	244·6	0·49	7·51
70.	Σ 2120, 210 Herculis	17 0·0	+28 15	6 ; 9	248·1	6·09	8·67
71.	Σ 2173, 221 Ophiuchi	17 24·5	− 0 58	6 ; 6	343·1	0·84	10·69
72.	β 631, 255 Ophiuchi	17 33·0	− 0 25	7 ; 7	238·8	0·40	8·51
73.	OΣ 338	17 47·0	+15 21	6½ ; 6½	21·2	0·84	9·73
74.	Σ 2262, τ Ophiuchi	17 57·1	− 8 11	5 ; 6	255·2	1·72	8·71
75.	Σ 2272, 70 Ophiuchi	18 0·0	+ 2 33	4 ; 6	336·5	2·01	10·61
76.	Σ 2315	18 21·0	+27 20	7 ; 8	226·5	0·27	7·56
77.	OΣ 358	18 31·0	+16 50	6½ ; 7	198·4	1·73	8·74
78.	γ Coronæ Australis	18 59·0	−37 14	5½ ; 5½	188·6	1·78	8·67
79.	β 248	19 12·9	+22 51	6 ; 9	125·1	1·70	8·68
80.	Schjellerup 30	19 21·5	−12 22	8 ; 8	328·3	1·60	8·54
81.	OΣ 387	19 44·5	+35 2	7 ; 8	354·6	0·55	8·76
82.	OΣ 400	20 6·8	+43 37	7 ; 8	140·1	0·25	7·92
83.	OΣ 406	20 16·4	+45 1	7 ; 8	108·9	0·49	7·51
84.	β 151, β Delphini	20 32·4	+14 13	3½ ; 5½	310·1	0·29	8·65
85.	OΣ 413, λ Cygni	20 43·1	+36 8	5 ; 7½	70·5	0·66	9·86
86.	Σ 2729	20 46·0	− 6 2	6½ ; 6½	170·5	0·53	7·82
87.	Σ 2737, AB	20 54·0	+ 3 53	6 ; 7	283·5	0·90	9·82
88.	Σ 2758, 61 Cygni	21 2·0	+38 13	5 ; 6	121·2	20·58	8·84
89.	OΣ 535, δ Equulei	21 9·0	+ 9 34	4½ ; 5	193·1	0·2±	9·82
90.	β 838	21 15·3	+ 2 40	7½ ; 9½	95·8	1·71	7·77
91.	Σ 2799	21 23·3	+10 36	7 ; 7½	302·9	1·35	9·94
92.	β 170	22 3·0	−19 1	8 ; 8	60·4	1·56	8·78
93.	Σ 2909, ζ Aquarii	22 23·2	− 0 36	4 ; 4	324·3	3·04	10·79
94.	OΣ 489, π Cephei	23 4·4	+74 47	5 ; 7½	30·0	1·05	9·73
95.	β 80	23 13·0	+ 4 48	8 ; 9	319·5	0·84	8·79
96.	β 854	23 19·0	+ 5 26	8½ ; 8½	88·3	1·89	8·82
97.	Σ 3046	23 51·0	−10 7	8 ; 8	246·9	2·98	8·50
98.	β 733	23 56·7	+26 30	6 ; 9	32·5	1·16	8·67
99.	β 281	23 57·1	+ 1 31	8 ; 9	209·0	1·35	8·89

In the column headed "Observer," the following abbreviations are used:—

L. Leavenworth.	Te. Tebbutt.	M. Maw.	C. Comstoc
S. Schiaparelli.	β. Burnham.	H. Hough.	
HΣ. Herman Struve.	E. Engelmann.	T. Tarrant.	

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 171. JANUARY. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, December 12, 1890.

Lieut.-Gen. J. F. TENNANT, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. KNOBEL and A. M. W. DOWNING, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Downing. Seventy presents have been received since the date of the last meeting. The only ones calling for special mention are:—Ahmed Moukhtar Pasha, The Reform of the Turkish Calendar (in Turkish), presented by Sir J. W. Redhouse; and Celestial Photographs (glass positives and paper prints) taken at the Sydney Observatory, presented by Mr. H. C. Russell.

The thanks of the Society were returned to the donors.

On the motion of Mr. Ranyard, A. Cottam, A. J. Lecky, and W. H. Maw were appointed Auditors of Treasurer's Accounts for the year 1890.

Father Cortie read an abstract of his paper on the "Spectra of Sun-spots in the region B to D," the observations having been taken in the years 1882–89. The spots observed are distributed over both the disturbed and quiet periods of solar activity in the last cycle. A long table contains a list of nearly 300 lines observed, a mean widening being deduced for each line in both periods. The number of times each line was recorded among the more widened or most widened lines is also noted, and a table of remarks is appended. The following were among the chief results:—With regard to the iron lines the observations show that during the disturbed period (1882 to the autumn of 1886) they were very rarely entered among the most widened lines, but during the quiet period these lines were affected in sun-spots to a much greater extent. A curve has been drawn which exhibits those results. On the contrary, some titanium lines, and those more particularly which are faintest in the ordinary spectrum, have very frequently been observed among the most widened lines, both in the disturbed and quiet periods. Calcium is more or less steady in all the observations, the mean widening of pure calcium lines being low. With regard to lines due to unknown substances, by far the majority in this part of the spectrum, the observations bring out the following facts:—During the maximum, when large spots are abundant, many new faint lines make their appearance; such lines are not characteristic of the disturbed period, but have reappeared in the larger spots of the quiet period. There are some faint lines among them, lines unrecorded by Ångström, which are constantly widened in all spots large or small and of whatever period. Spot-bands, which were observed in 1885 and 1886 on eight occasions, disappeared with the advent of the quiet period. Although the "basic" lines, of which eleven examples occur in this part of the spectrum, have all appeared among the most widened lines at one time or another, yet such appearances, when contrasted with the whole number of observations, have been comparatively rare, but 15 times in a total of 407 observations. Several of the lines marked as telluric by Ångström have been seen widened in sun-spots.

Mr. Maunder. For some short time I was engaged in a similar work in a different part of the spectrum, but only in one of the two periods of which Father Cortie speaks—that of maximum. So far as my observations went they quite confirmed one or two of the points which this paper brings out. One of the most interesting points is that the bands which are seen in the spot-spectra are typical of the maximum period. The bands which I saw in the green were very conspicuous in the period just before the maximum, and they seemed to get less and less conspicuous as the activity of the Sun decreased. Father Cortie found that the spot bands in the red end of the spectrum faded out entirely in the minimum period. Mr. Lockyer, at South Kensington, using a different part of the spectrum, found that the iron lines were connected with the sun-spot period, and in 'The Chemistry of the Sun' gives details showing how the number of most widened iron lines decreases towards maximum and increases towards minimum. Father Cortie's results confirm this, but while Mr. Lockyer found that lines of unknown origin vary in a manner almost exactly the opposite to the iron lines; that is to say, as the iron lines decrease in number, so the lines of unknown origin become more conspicuous, this is not the case in Father Cortie's observations. If the two curves are so exactly opposed to each other as Mr. Lockyer finds them, there seems to me a suggestion of some connection between the two. Mr. Lockyer also laid considerable stress on the fact that all the basic lines in some parts of the spectrum are much affected. Father Cortie found that though nearly all the basic lines in his part of the spectrum were affected at one time or another, yet out of the vast number of observations (over 400) the basic lines were only 15 times widened. These results would therefore very largely destroy the effect of Mr. Lockyer's argument based upon the fact that the basic lines were so strongly represented in spot-spectra.

Mr. Ranyard. I should like to congratulate Father Cortie upon the number of general results he seems to have produced. There is one point which struck me as curious. He says that some of the atmospheric lines are widened. It is rather difficult to judge of the widening of lines in a sun-spot as compared with the photosphere section without getting the actual measurement, because there is a great deal in the physiological effect of the brightness of the background. This may be seen by dimming any part of the spectrum by putting a match in front of the slit. It is very difficult to compare the breadth of lines in a comparatively faint spectrum with those in a bright spectrum. With regard to the basic lines mentioned, there is a double chance of their widening, since they are due to two metals with lines apparently coincident.

A vote of thanks was accorded to Father Cortie for his paper.

The President. Mr. Roberts is going to show us some stellar photographs with the aid of the limelight and an apparatus which we have purchased for the purpose.

[Mr. Roberts then exhibited and explained his various photographs.]

The President. I am sure everybody here will join in giving most cordial thanks to Mr. Roberts. We have all seen and learned something that we did not know before. We also owe a vote of thanks to Dr. Spitta for the trouble he has taken in supplying us with the apparatus by means of which we have been able to see these interesting sights. The scientific meeting is now adjourned until 9th January, and we will proceed to the special meeting, of which notice has been duly given.

SPECIAL GENERAL MEETING.

Mr. Downing. The following notice has been sent out to the Fellows: "A Special General Meeting will be held on Friday, 12th December next, at 9 o'clock precisely, after the ordinary evening meeting. Business proposition: 'That henceforth for the period of one year the meetings of the Society be held at 4.30 P.M., and that Bye-law 44 be altered accordingly.'"

The President. I must ask that strangers and visitors will withdraw, as the proceedings are now devoted to the ordinary business of the Society.

Mr. Knobel. I beg to move that the reporters be allowed to remain.

Mr. Downing. I beg to second that.

The motion was carried.

Mr. Chambers. Mr. President, I have ventured to bring forward this motion on the ground of doing the greatest good to the greatest number, and I have done so because I think the discussion which has already taken place on this subject on former occasions somewhat inadequate. Sir Erasmus Ommaney, who was to have seconded my motion, is not here, but he has written to me to say, "You may say I am certainly in favour of afternoon meetings. In other societies where the change has been adopted it has given great satisfaction." When the Society was founded in 1820, the common times for dinner were 4 o'clock or 5 o'clock, and everybody was obliged to sleep in London, because there were no facilities for going away at night. Coming down to a later period, to 1856, the common times for dinner were 6, 6.30, or 7, but now we are face to face with a different sort of thing. The great extension of railways within London, and the development of the London season, have pushed on the ordinary dinner hour from 6.30 to 7.30 and 8. I have in my motion put down the hour for meeting as 4.30, but, supposing 5 o'clock is suggested, I should not wish to stand out, supposing the Society is willing to have the meetings at that hour for a year. It would enable Fellows living within 100 miles of London to be present without sleeping in London. Five o'clock, or thereabouts, would enable Fellows going home from the City or from business to attend here and yet get home for a quiet evening. The ordinary business of the Fellows of this Society being over by 5 o'clock, they have with our present hour of meeting no chance of dining with friends, and they come here more or less dinnerless. Again, in winter there are not the same inducements to spend the evening here. These practical reasons, arising out of the altered circumstances of the times in which we live, justify me in asking the Society to assent to this change. Wherever this change has been adopted in other London societies, it has been appreciated. I may mention that the British Astronomical Association has adopted the early hour. I will therefore formally propose my motion to the meeting. Sir Erasmus Ommaney not being here, I must ask some Fellow to be good enough to second it.

Mr. Knobel. What is your motion?

Mr. Chambers. "That henceforth for the period of one year the meetings of the Society be held at 4.30 P.M., and that Byelaw 44 be altered accordingly."

Mr. Cushing. In order that this motion may be properly discussed I have great pleasure in seconding it.

Mr. Knobel. I rise to a point of order. I asked Mr. Chambers the particulars of his motion, thinking that probably he would have seen fit to make some modification. As the motion stands at the present time, it is that the Fellows should alter the Byelaws for a limited period—for one year. That is to say, that after one year the Bye-laws should revert to their present position. That is a motion that is entirely out of order and cannot be put from the Chair. It is impossible to legislate for the future. Besides this, the motion amounts practically to the alteration of a Bye-law at the end of the year without any general meeting. I raise that point of order, Sir, for your decision; but it seems to me that the resolution is out of order, and cannot be put from the Chair.

Mr. Freeman. I shall be glad to move an amendment.

The President. Mr. Knobel's objection is a sound one, as there is considerable difficulty in altering a Bye-law for a time. I think the best thing to do will be to alter it permanently, but I do not think you can alter it temporarily.

Mr. Freeman. I propose that we strike out the words "for a period of one year." As the President has remarked, a general meeting is always capable of reversing what a general meeting has decided. If a general meeting decides it is not advisable to hold afternoon meetings, we can go back to our present condition. A Fellow who attends the meetings has the advantage of being able to question the authors of the papers, and certain Londoners wish to deprive country Fellows of this advantage entirely. [Cries of "No!"] I found that the last time the question was discussed, the London Fellow was everybody and the country Fellow nobody. I cannot afford the expense of coming to these meetings regularly. It costs me 25*s.* to come, and as a country clergyman I cannot afford it; but if we meet at 4.30 it will only cost me 10*s.*, and I may be able to afford that. To come and hear the papers is much more instructive than reading any of the admirable reports we get. There are demonstrations on the blackboard which the reporters cannot take down, and which men coming here can appreciate, and these admirable photographs and illustrations cannot be repeated in the 'Observatory.' This question was first started by Mr. Sydney Waters, and has been left to independent Fellows to bring forward since then.

Mr. Stone. I will second the amendment. It appears to me simply to be a question of the country members. We want to consult the greater convenience of the greater number of members. I do not think any quantity of argument would prove whether afternoon or the evening is more convenient, but if each member will vote straight for his own convenience, probably we shall arrive at some decision.

Mr. Common. There is one point that Mr. Chambers has left out.

The President. Are you speaking to the amendment?

Mr. Common. No—to the general question; but my speech will do equally well for the amendment. [Laughter.] The point I want to put before the Meeting is this—Mr. Chambers has brought forward some arguments for the alteration of the hour, which ought not to weigh with astronomers; but he has omitted this point, that on a fine night we lose the opportunity of making observations.

Mr. Turner. I oppose the amendment for many reasons. I agree with Mr. Freeman that there is a distinct advantage in attending the Meeting, and I do not think any of us dispute it; but we have to ascertain which of two parties we shall favour. No doubt by adopting Mr. Chambers's motion we shall exclude a certain number of members from attending regularly who now attend, but, at the same time, we admit others who do not attend. The only question is which of these parties we shall favour. Now the form in which the original motion is cast is superior to that of the amendment, as it gives us the opportunity of making the experiment for a year, which I think would be extremely valuable.

The President. We cannot alter a bye-law for a limited time.

The Astronomer Royal. If the original motion is ruled out of order the amendment takes its place, and, therefore, we can speak on it.

The President. The question is, shall these words be erased from the resolution or not?

The Astronomer Royal. If the original resolution is out of order, the amendment is out of place.

Mr. Knobel. The rule is that the Fellows of the Society shall give permission for the amendment to be put.

The Astronomer Royal. Would it not be better to put it?

Mr. Ranyard. I propose that the amendment be not put, because, if it be put and carried, the question will be discussed at an afternoon meeting, and those who cannot attend an afternoon meeting will not be able to come, because our future meetings will be in the afternoon.

Mr. Knobel. If the amendment is carried, we then have a substantive motion before us.

The President. The question is, shall the words "for the period of one year" be struck out of the motion?

The Astronomer Royal. I do not think you have to vote on that point yet, the question is whether this amendment is to take the place of the substantive motion. The substantive motion is out of order. What we want to have is something in its place to discuss.

Mr. Mitchiner. I beg to submit that the Astronomer Royal is out of order. If the original resolution is out of order, you cannot put an amendment, except to put to the Meeting that the words be left out, but no amendment can be put to the resolution so long as you rule it is out of order.

Mr. Chambers. I beg to withdraw the words you have ruled out of order.

The President. Will the seconder agree to that?

[Leave was then given to withdraw the words "for the period of one year."]

The President. Now we have this resolution without the limit.

Mr. Turner. I will speak to the original motion, the original motion without these words in. I would ask you whether I should be in order in suggesting that a vote be taken on the question in the spirit of the original motion rather than the letter of the present one. That is to say that I and a seconder will pledge ourselves to raise this question at an annual meeting, where, I think, the business of the Society should be properly decided. Shall we be in order in giving that pledge before the vote is taken? because I consider that the vote on this occasion, if it is distinctly understood that the change is more of an experimental nature than a final one, will be given in a very different spirit. If it is distinctly understood that the relative advantages are to be balanced before the Society is committed as a body to a certain course of procedure, the vote will be given in a much more liberal spirit. I ask whether I shall be in order in making the suggestion that, at the next Annual General Meeting but one, we pledge ourselves to raise this question again.

Mr. Common. I will undertake to second Mr. Turner's motion.

Mr. Knobel. It is impossible to give a pledge of that sort. The proposer and seconder may not be here to fulfil their pledges.

The President. We cannot take it as a definite promise; still it is one of those promises that are given in good faith amongst gentlemen, and we can quite understand that the question will be revived at that time one way or another. The resolution before the Meeting now is, "That henceforth the Meetings of this Society be held at 4.30 P.M., and that Bye-law 44 be altered accordingly."

Mr. J. G. Petrie. I opposed this resolution on the last occasion that it was raised, and, though only a young member, I am inclined to oppose it again. I think it is a very dangerous thing to make any change in the hour of meeting. I have had experience of changing the hour in similar learned societies to oblige country members, and the experience has been that they have come up once or twice, but after that they have not come any more. The feeling of the country members is more a sentimental than a practical one, and the attendance at the meetings of learned socities in London must always almost entirely depend upon London men. I do not say that in any unkind way to debar country members coming to London; but if they are unable to come they must be very well pleased to see the printed proceedings and content with the honour of belonging to the Society. Mr. Chambers mentioned the earlier hour which the British Astronomical Association has adopted; but I believe that was done to a large extent to oblige the lady members. I would ask you, before you change the hour, to be certain that it will do good and that it will bring country members here.

Mr. Waters. As the proposer of this resolution on a former occasion, I should like to say, from inquiries I have made amongst the members of the Royal Society, that their opinion is that a very distinct improvement in the attendance at that Society has occurred since the change of their hour of meeting. Under those circumstances I imagine there is no reason why the same thing should not happen with us. Moreover, I think there are many of the older members of this Society whose presence we should be glad to have, but who very often are debarred from coming in the evening, whereas they might be more willing to come in the afternoon. I beg, therefore, to support the motion.

Mr. Seward. As a town member I should like to appeal to the mover and seconder to alter the hour to 5 o'clock. I am engaged in the City, and could get here about a quarter past 5. I have my dinner about 6 o'clock, and I do not feel inclined to turn out at 8 o'clock. I will not move an amendment, but will vote for the resolution if 5 o'clock is substituted for 4.30; if not, I shall vote against the resolution.

Mr. Knobel. Fellows will doubtless bear in mind the decision arrived at by the Council, and remember that the business can be properly conducted if this change is made; but the business of the Council generally occupies about two hours, and if we had an ordinary meeting immediately following that, the officers of the Society would be engaged for 4½ hours without intermission. It is for you to consider whether that would be fair to them or whether it would be a tax upon them. I am sure the officers of the Society will be found quite equal to any amount of extra strain that the Fellows may decide to put upon them. The arguments which have been brought forward in support of the motion seem to me singularly few and weak. Mr. Chambers's principal argument is the change in the dinner hour that has taken place since 1820. This change was probably due to the fact that when the Society was founded men went to business very much earlier and finished business very much earlier than they do now. This Society is almost a society of amateurs, and in making arrangements we have to consider how far they will fit in with the customs of amateurs. The majority of amateurs are in business; and it is hardly to be supposed that they can attend meetings at 4.30 without interfering with their business occupation. There is another thing which has been of very great value to Fellows which, if this motion is carried, will be knocked on the head entirely, and that is the very pleasant half or three-quarters of an hour which we spend after the Meeting. That feature of our Society which has been the means of doing so much for the prosperity of Astronomy will be banished altogether. These are some considerations which lead me to hope that the resolution will not be passed.

Mr. Ranyard. I like to go when I can to the Physical Society, but my business engagements make it often impossible, and if our meetings were continued on the second Friday in the month we should clash with the Physical Society, and there are a great many members of that Society who are members of the R. A. S.

Mr. Mitchiner. Between 4.30 and 5 o'clock is merely a technical distinction of little moment. Above the convenience of the professional or amateur astronomers or even of the Council, we must consider the interests of the R. A. S., and Mr. Chambers failed to convince me of any necessity in the interests of the Society for making the change. It is essentially an experiment, and as such I think it would prove a mistake. The Royal Society stands pre-eminently by itself. I apprehend that the same objection which would appertain to the Geographical or to the R. A. S. would not appertain to the Royal Society.

The Astronomer Royal. To remove any doubt as to whether those who favour a change of hour would like 4.30 or 5 o'clock, I move that the words "5 o'clock" be substituted in Mr. Chambers's resolution for "4.30." I have no very strong feeling on the matter, but it strikes me there is one point which ought to be borne in mind. Mr. Knobel has enlarged upon the loss we should sustain in that social gathering which takes place after the meeting. In the case of the Royal Society they have a gathering before the meeting, and have afternoon tea, and I think that is much more in accordance with modern social habits than afternoon tea at 10 o'clock at night.

Mr. Knobel. How will that fall in with the Council Meeting?

The Astronomer Royal. The meetings of the Royal Society are at 4.30. As a matter of fact, the Royal Society found it possible to hold the Council Meetings and get through a great deal more business than our Council does before 4 o'clock, and they are able to enjoy social intercourse and discuss scientific questions in the half-hour then available, and a great deal of important work is done in the progress of science generally. I merely put that before the Fellows of this Society as an example. I do not wish to say the R. A. S. is exactly the same as the Royal Society, but I think Fellows are drawn from substantially the same class. There are a great many professional business men in the Royal Society who cannot come early in the afternoon, but they come to these meetings. I merely move the alteration from 4.30 to 5 o'clock, in order that Members may express their sense upon the matter in case the hour is changed.

Prof. A. S. Herschel. I think Fellows should remember the original aim of the motion, which was to afford facility to Members living in the country of attending the meetings of the Society. If we are going to adopt the afternoon hour, let it be done fully.

Mr. Lecky seconded the alteration from 4.30 to 5 P.M.

Mr. Chambers. I will withdraw 4.30, because I think 5 o'clock will reconcile those who wish for an earlier hour.

Mr. Bryant. As one to whom the change of hour would make very little difference, I would ask would it not be preferable, as on a previous occasion, to send reply postcards or single postcards to all members asking their opinions?

The President. Notice has been given that the proposition would be made to-night, and we could not do anything more if we sent 50 postcards to each member.

Mr. Bryant. But it would allow each Member to vote by post.

The President. We tried it, and the result was endless confusion. I think the simplest way will be to put the Astronomer Royal's motion.

Mr. Roberts. I know one gentleman who does not attend these Meetings because he could not get home at night. If you made it 4.30 he could, but if it was made 5 o'clock he could not.

The voting then took place, when the President declared that 19 voted for 5 o'clock, and 17 for 4.30. The resolution was then amended to 5 o'clock.

Mr. Knobel. The Resolution is now:—That henceforth the Meetings of the Society be held at 5 o'clock P.M., and that Bye-law 44 be altered accordingly."

On a vote being taken, 18 voted for and 30 against, and the Resolution was lost.

Mr. Chambers. Now I beg to demand a poll of the entire body of Fellows, and I would suggest that that should be taken by postcard. [Cries of "No!"] I would point out that it is the universal law of every Company and Society in England, that whenever a motion is rejected or carried on a show of hands, the minority are, by the common law of England, entitled to demand a poll.

The President. Not in this Society; there is nothing in our statutes or bye-laws for such a course. Can you show me a precedent?

Mr. Chambers. The common law of England, 700 years old.

The President. There is no precedent in the Minutes of the Society.

Mr. Ranyard. The bye-laws are directly to the contrary. It is said the bye-laws shall not be altered except by vote of General Meeting, or Special Meeting duly called. A canvass by postcard would not be an alteration of bye-laws in the way proposed by the bye-laws of the Society.

Mr. Chambers. Then, if I am debarred the opportunity of learning the wishes of the 500 absent Fellows, I shall take the opportunity of re-opening this question at the Annual Meeting in February next.

The following Candidates were proposed for election as Fellows of the Society:

[illegible], Surveyor of the Post Office, [illegible] Lansdowne Road, Didsbury, Manchester (proposed by E. J. Spitta); *Shin Hirayama*, Astronomer at Tokyo Observatory, Japan (proposed by W. H. M. Christie); *[illegible]*, Professor of Mathematics at Durham University (proposed by Rev. T. E. Espin); *[illegible]*, St. John's College, Cambridge (proposed by Dr. [illegible]).

Rev. John [illegible], [illegible] Parkgate Road, Chester, was duly elected a Fellow.

The following were balloted for and duly elected Associates :—

Lewis Boss, Dudley Observatory, Albany, N.Y., U.S.A.; *A. Cornu*, Paris; *C. Souillart*, Lille.

The following papers were announced :—

H. C. Russell. "Note on Celestial Photographs taken at Sydney Observatory."

W. F. Denning. "New Nebulæ."

S. W. Burnham. "Note on Hind's Variable Nebula in Taurus."

A. Marth. "Ephemerides of the Satellites of Saturn, 1890–91."

J. R. Hind. "Elements and Ephemeris of Comet Zona."

Rev. A. L. Cortie. "Observations of Spectra of Sun-spots in the Region B to D at Stonyhurst College Observatory, 1882–89."

MEETING OF THE ASTRONOMICAL SOCIETY OF THE PACIFIC.

November 29, 1890.

The Meeting was held at 232 Sutter Street, in the Young Men's Christian Association Hall, the President, E. S. Holden, in the Chair.

The Minutes of the last meeting were approved.

At the meeting of the Directors 32 new members were elected; the total membership of the Society is now 312.

The following papers were presented to the meeting :—

(*a*) "The Law of the Solar Corona." By Prof. Frank H. Bigelow, of the Nautical Almanac Office, Washington, D.C.

(*b*) "Coronal Extension." By C. M. Charroppin, S. J., University of St. Louis, Missouri.

(*c*) "Observations and Drawings of Saturn, 1879 to 1889. By Prof. Edward S. Holden, Lick Observatory,

(*d*) "The Observatory of Swarthmore College." By Miss S. J. Cunningham, Director.

(*e*) "The Kenwood Physical Observatory (Chicago)." By George E. Hale, Director.

(*f*) "Work at the Lick Observatory, 1888 to 1890." By Prof. Edward S. Holden, Director.

(*g*) "An account of an experiment made to determine whether Gravitation Force varies with the Temperature." By A. E. Kennelly, of Orange, N. J.

(*h*) Index Map of the Moon. By Prof. C. A. Young.

Prof. Holden gave a popular account of the work of the Lick Observatory since the time the Observatory has been in the hands of the astronomers.

The meeting then adjourned.

It is hoped that the next meeting (January 31st) can be held in the Society's rooms in the Californian Academy of Sciences' new building, which is not yet completed.

CHARLES BURCKHALTER, *Secretary.*

MEETING OF THE LIVERPOOL ASTRONOMICAL SOCIETY.

The third ordinary Meeting was held on Monday, December 1, at half-past seven o'clock, in the Royal Institution, Colquitt Street.

W. B. Hutchinson, Esq., F.R.A.S., President, in the Chair.

The Rev. Fred. F. Grensted, M.A., gave a lecture on the "Airless and Waterless Condition of the Moon," the first part of which was illustrated by numerous views exhibited by means of the lime-light. Equal areas of the Earth's and Moon's surface were shown by means of photographs, of Nasmyth's plaster cast of the lunar surface, and of an albo-relievo map of Asia, and the striking contrast of their configurations dwelt upon.

The Earth's surface being diversified by mountain-ranges and river-systems of drainage, exhibiting clearly marks of denudation and fluviatile erosion, the mountain upheavals having been subject to great lateral pressure. On the Moon's surface, on the other hand, there is nothing that really corresponds to our mountain-ranges or river-systems, and no evidence of denudation or fluviatile erosion, the most superficial investigation of the lunar surface pointing in the direction of an airless and waterless condition of the Moon throughout its entire history.

The whole problem finds its solution in the difference of *size* which regulates the rate of cooling, and hence determines chemical differentiation—the greater mass retaining free oxygen, which forms our water and air, the lesser mass using it all up in oxidizing its crust, none being left free, and therefore no water and no air can be formed, assuming the Moon to be composed of similar material and on precisely the same type as the Earth. The Moon being, roughly speaking, only one fourth of the Earth's diameter and one eightieth of its bulk, it has therefore a very much greater surface in proportion to its bulk, and would cool with a corresponding degree of quickness. By the pressure of gravitation a pure metallic nucleus would be formed, possibly of iron, when the heat was still above the point of dissociation—that is, the point admitting of oxidation; hence the nucleus would be pure metal with an outer crust of oxidized metal, which would form as the temperature was reduced and prevent any further oxidation inwards. The Earth having a density of $5\frac{1}{2}$ that of water, while its surface rocks only equal about $2\frac{1}{2}$, must have a nucleus of $7\frac{3}{4}$ times the density of water, and have an oxidized crust one sixth of its radius in depth; while the Moon having a density of $3\frac{3}{4}$ times that of water must be oxidized to nearly a half of its radius from the surface and so have used up all its oxygen, so that none is left free to form water or air.

A vote of thanks having been accorded to the lecturer, an interesting discussion followed.

Several papers were taken as read and will appear in the third number of the Journal.

MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

At the meeting held on Dec. 17, Mr. R. H. Scott read a paper on an interesting lightning-stroke which struck a house near Ballyglass, Co. Mayo, on Jan. 5. Considerable damage was done to the house, and a remarkable fate overtook a basket of eggs—the shells were shattered so that they fell off when boiled, leaving the inner membrane sound; those at the bottom of the basket were "flattened but not broken."

Mr. A. Brewin related the damage done to his house at Twickenham by lightning on Sept. 23.

Captain Hepworth, in a paper on the Wind Systems between the Cape and Australia, recommended mariners to run down their longitude between parallels 41 and 42 in winter, and 45 and 46 in summer. Mr. Mawley stated that the first months of the year could not have been more favourable for vegetation, but that July and August were quite the reverse. Dr. Doberck sent a paper on the climate of Hongkong, 1884–88.

Bright-Line Stars of the Wolf-Rayet Type[*].

Three varieties of gaseous stars can be at once distinguished. The first kind are white in colour, and show the bright lines of hydrogen and helium independently of variations in their total light. The classical examples are γ Cassiopeiæ and β Lyræ; but ϕ Persei, P Cygni, Pleione, δ and μ Centauri, π Aquarii, and some others, belong most likely to the same category. The second includes red stars with banded spectra, extensively variable after the fashion of Mira and χ Cygni, and developing bright lines concomitantly with their luminous accessions. These lines, although identical, broadly speaking, with those of the previous variety, originate possibly under different conditions. It is at least certain that the vivid emissions of Mira have their seat close to the photosphere; the stopping-out of the hydrogen H by the widened dark H of calcium, *teste* the Harvard photographs, being so far conclusive as to the order of atmospheric stratification that the emitting hydrogen must lie *below* the absorbing calcium. But there is no proof that an analogous state of things prevails in such stars as Pleione and γ Cassiopeiæ. Rather the reverse seems indicated by the superposition of thin bright hydrogen lines upon bands both diffuse and obscure.

In bright-line spectra of the third variety, on the other hand, the rays of hydrogen and helium occur, either subordinately or not at all. The chemical significance of their main features is unknown. Among these a yellow and a blue band are especially conspicuous; and it was to test a supposed coincidence of the latter with the hydrocarbon band beginning at λ 474, that the investigation, of which the results are now before us, was undertaken by Dr. and Mrs. Huggins.

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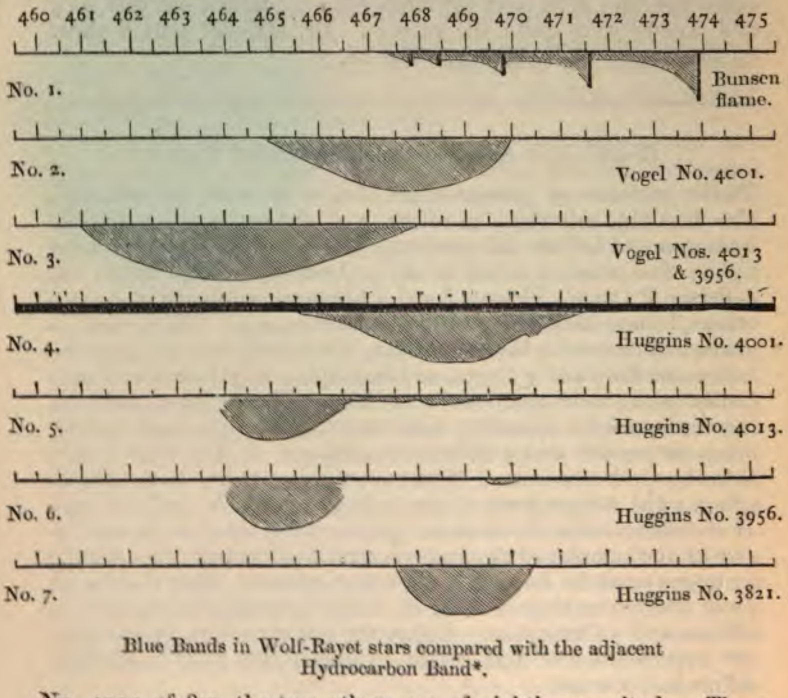
[*] "On Wolf and Rayet's Bright-Line Stars in Cygnus." By William Huggins, D.C.L., LL.D., F.R.S., and Mrs. Huggins. Paper read before the Royal Society, December 11, 1890.

Four stars, all in the constellation Cygnus, were examined by them, viz.:—

B.D. +35° No. 4001
B.D. +35° No. 4013
B.D. +36° No. 3956
B.D. +37° No. 3821

The first three are the original members of the class, discovered by MM. Wolf and Rayet in 1867; the peculiar character of the fourth was detected by Dr. Copeland in 1884, and two years later photographically by Professor Pickering. It is of seventh, No. 4013 of 8·5, the two others are of eighth magnitude. The spectra of all are intimately related, yet present important differences, recorded indeed with his unfailing accuracy by Dr. Vogel



Blue Bands in Wolf-Rayet stars compared with the adjacent Hydrocarbon Band*.

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* The unfinished shadings in Nos. 5 and 6 represent inability to determine with certainty how far the light could be traced *upwards* in the spectrum.

at Vienna in 1883, but only now clearly defined and satisfactorily discussed.

The blue band owns, in none of the four stars, the character of a true fluting; its aspect is rather that of a congeries of very close bright lines; and it varies strangely in position. The two very different situations relative to the blue band of the Bunsen flame, assigned to it by Vogel, are shown in Nos. 2 and 3 of the accompanying diagram; they were substantially confirmed by the measures at Tulse Hill, represented in Nos. 4, 5, and 6. The brilliant band proved notably more refrangible in the stars 4013 and 3956 than in 4001, and with the latter the more luminous object No. 3821, now for the first time submitted to detailed spectroscopic scrutiny, was in this respect found to range (see Diagram, No. 7).

The difference is abrupt and evidently systematic; but a connecting link is perhaps afforded by the singularly wide band in Arg. Oeltzen 17681, which, according to Vogel, covers the places of both the narrower bands in the other allied specimens, rising to a maximum, as our authors remark, at λ 466, where the two qualities of radiance overlap. Nor is the normal obliteration of one of the alternative bands absolutely complete. In stars fully developing the more refrangible, traces of the less refrangible member of the pair were perceived by Dr. and Mrs. Huggins, and *vice versâ*, leaving no doubt of their close genetic relationship. "If the two bright groups," they add, "differing in position by about λ ·0040, belong to different substances, or, less probably perhaps, to different molecular conditions of the same substance, it is conceivable that one or other substance, or molecular state, may predominate and appear brilliant, according to certain unknown conditions which prevail in the stars' atmospheres." The suspicion further expressed (though with much reserve) that the two bright blue groupings may prove relatively variable, challenges careful future verification.

A glance at the diagram is almost sufficient to dispel the idea that carbon can have any share in producing them. Their structure and place in the spectrum are alike inconsistent with the identification. In stars 4013 and 3956 "the great body of bright radiation" (it is remarked in the paper under discussion) "lies far beyond the ordinary visible limit of the blue carbon-band, and no connection whatever with carbon is even suggested." The effulgence in the companion-objects, however, culminating in brilliancy between λ 468 and λ 469, considerably overlaps the carbon-band; and it happens that, under certain electrical conditions, the brightest part of the latter is shifted from the first fluting at λ 474 to the third fluting at λ 470, so that the two maxima, such conditions being present, do not lie very far apart. But that the disagreement is real and irreconcilable becomes obvious from the consideration that the first line in the carbon-band retains, even when shorn of its primary importance, sixsevenths, and the second line eleven-twelfths, the enhanced intensity of the third line; while the stars show absolutely no luminosity corresponding to the first, and, if any, a bare trace corresponding to the second of these still conspicuous flutings. Moreover, the blue carbon-band is one of a series, and the blue star-band stands alone. Not only are the green and yellow flutings, inseparably associated with the blue in the hydro-carbon-spectrum, absent from the stellar spectra, but those occurring in the indigo near λ 431 and λ 438 make no appearance in Harvard photographs of Wolf-Rayet 4001, distinctly imprinted with the hydrogen line at λ 434.

The representation in them of the hydrogen-set, up to and including Dr. Huggins's α, is complete; yet F, although seen as "tolerably bright" by Vogel in 1883, remained imperceptible to our English observers in 1890. It showed, nevertheless, brilliantly in No. 3821, the spectrum of which was not previously known to include it; the suspicion being hence raised of its variability in these two, as in other noted instances. Hydrogen-emissions have been recorded in one other star of the Wolf-Rayet type—Lalande 13412; and all three have in common the additional feature of displaying their characteristic blue band in the less refrangible of its two forms. An opposite correspondence is perhaps indicated by the absence of bright hydrogen from stars in which the blue band is placed higher up at λ 464. Besides the pair in Cygnus, Nos. 4013 and 3956, γ Argûs, Stone 9168 in Scorpio, and two faint analogous objects in Argo and Centaur, appear to combine these two peculiarities. In one of them (No. 4013) Dr. and Mrs. Huggins were just able to perceive, near the place of D_3, a faint bright line, which will most likely prove identical with the feeblest and least refrangible of those shining in γ Argûs. Its precise position has yet to be determined; but a genuine coincidence with D_3 may be anticipated from the analogy of nebulæ, in the spectra of which that enigmatical ray has now an established place. It is true that the presence of helium without hydrogen might seem anomalous; yet the appearance of bright sodium (which may be called the alternative supposition) would be, in a sidereal object, unprecedented. Sodium was, at any rate, proved by direct comparison at Tulse Hill *not* to be responsible for a bright line in star No. 4013 falling near the green sodium-pair at λ 5687 and λ 5681. With high dispersion it was resolved into "a short group about λ 0024 in length of several bright lines close together, and nearly equally bright," crossed by the sodium-pair "at about one-fourth to one-third of the length of the group from its more refrangible end."

These recent observations of the Wolf-Rayet stars disprove, it may safely be said, conclusively, the admixture of hydro-carbon radiations with the remaining elements of their light; on other points, neither time nor optical means were available to render them exhaustive. We learn, however, that the objects in question possess "a fairly luminous continuous spectrum, which can be traced past the blue band into the violet, apparently as far as the eye could be expected to follow it." Although in many parts irregularly encroached upon by absorptive, as well as intensified by emissive effects, it showed, to the most attentive scrutiny, no brightening at the place of the chief nebular ray. Enquiries as to the existence of nebulosity about the stars had similarly a negative upshot. To the practised eyes of Messrs. Burnham and Barnard, of the Lick Observatory, they seemed quite sharp; and their strong impressions upon a plate exposed by Mr. Roberts for two hours on November 1st, 1890, are purely stellar.

Their nearest analogues are found, nevertheless, in planetary nebulæ. The closeness of the affinity has been rendered especially manifest through photographs taken in connection with the Draper Memorial, of fifteen nebular, and all the stellar spectra of this type hitherto discovered. No details of the comparison have yet been published; but it is stated that most of the nebulæ show the stellar blue band, and in the approximate position (λ 470) corresponding, it can be inferred, to their simultaneous display of hydrogen-lines. This peculiar quality of radiation, first observed in the annular nebula, Gen. Cat. 4964, by Dr. Huggins in 1864*, was measured, a few years later, as of λ 469 with the Harvard College fifteen-inch refractor †, and came out, from Dr. Copeland's determination of it in 1880, in the Stephen-Webb Cygnus planetary, as of λ 469·4 ‡. The alternative, more refrangible band at λ 464 has so far not been identified in any nebula. Hence the three stars Lalande 13412, Cygnus Nos. 4001 and 3821 would appear, by the possibly connected circumstances of the low position of the typical blue effulgence, and the emergence in their spectra of hydrogen-lines, to approach planetary nebulæ more closely than the other members of their species. The suggested inference that the order of resemblance traces a line of slow migration, is a plausible conjecture; but it is nothing more. Facts of distribution have to be taken into account. About twenty-seven stars of the Wolf-Rayet type are now known §; they all, without exception, occur in the densest parts of the Milky Way. But planetary nebulæ, although strongly preferring, are not limited to the Milky Way. Nor do these share the remarkable clustering tendency of the stars, of which seven are collected in a space of about three degrees square in Cygnus, five are similarly grouped in Scorpio, and two close pairs are met with respectively near γ and near η Argûs. This associative tendency sets them in a measure apart, and throws them, as it were, off the line of the great highway of development.

A. M. CLERKE.

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* Phil. Trans. vol. cliv. p. 440.

† Harvard 'Annals,' vol. xiii. pt. i. pp. 72, 81.

‡ "Copernicus," vol. i. p. 2.

§ I take this opportunity of rectifying the erroneous classification of θ Muscæ as of the Wolf-Rayet type in the Appendix to my recently-published book 'The System of the Stars.' All that is yet known of the spectrum of this star is that it includes a bright F, photographically registered at Chosica.

The Extension of the Corona, and the Details of its Structure.

THE communications of Professer Holden in the Publications of the A. S. P., May 1890, and of Mr. Taylor in the 'Observatory,' Nov. 1890, on the Eclipse operations of this year, involve material of such importance that I venture to make a few suggestions bearing upon the subject. Two questions should be kept well separated, and these are the photographing the corona in its extension and in its structural detail respectively.

The visible extension depends upon the actinic brightness of the corona at the time of the eclipse, and upon the atmospheric conditions of the station, both of which are variable terms; but in no case can it be assumed that we ever do see the whole extent of it, if we keep in mind the coronal forces as distinct from certain substances that trace the paths of these lines of force. Therefore the effort to secure photographs of its actinic waves of light has a definite limitation, that of the faintness of the skylight itself. Nor does it seem nearly so important to draw the boundary of this nebulous mass, as to photograph the course of the rays throughout it. Fortunately the experience of the recent eclipses shows that great extension is also compatible with admirable details of the rays.

Mr. Taylor's statement lacks definiteness in one vital point, for he uses as his central thought the expression, "if the density of the negative is *any* measure of the brightness of the light." It is admitted that lights of different intensities, exposed for the same time, produce a proportional density-deposit, but if the times of exposure vary, the density follows some logarithmic law, the modulus depending upon the sensitiveness of the plate, in which the proportionality, holding approximately for 8 or 10 seconds, falls off later, so that the fainter light relatively gains upon the other, the effect in regard to the corona being, that after a certain short exposure the skylight begins to fill up the details that ought to be unaffected, while the *extension* will not differ much in either case. The relation between the variation of density, due to the exposure of the same light for different times, lies at the foundation of our further progress, and should be solved before the next eclipse.

Having used the coronas of the Lick Observatory in my measurements, and seen the trumpet-shaped equatorial extensions in the photographs of January 1, 1889, I venture to think that the next step is to attempt to account for them, and the following suggestion is offered for what it is worth. It seems to be pretty clear that the so-called polar rays of the corona mark out the lines of force emanating from the Sun, referred to a potential distributed as a zonal harmonic. If this is so, there should be in space the corresponding equipotential surfaces, that is the surfaces upon which the material transported along the stream-lines will tend to accumulate. Upon a rather careful comparison of these trumpet-shaped lines with a diagram of equipotential surfaces of this kind, the resemblance seems to be striking, and any discrepancies may be accounted for by the projection of the saucer-shaped surface of revolution which the rotation of the equipotential lines would produce. The faintness of these curves was such as to preclude micrometric measures, and certainly to make fair prints of them would be very difficult, so that it will be interesting to obtain this detail once more at some future eclipse. If this view is correct, these lines are not to be regarded as streamers from the equatorial regions, but as projected surfaces generated from the potential forces.

There is, however, another way of looking at the general subject. Induced by certain motives, it has become customary to assume that extreme sensitiveness of the photographic plates is most desirable in an eclipse equipment. For some portions of the work it must be so, but a doubt may be raised against it when detail is to be aimed at. On the rapid plates, the skylight certainly has the most of an injurious effect in blurring the details, if the exposure is prolonged; and furthermore the granulation of the gelatine dry film surface is a serious obstacle to any magnification, either under the microscope or by enlargement. Long exposures on rapid plates, for example in a camera or mirror of say $a/f=\frac{1}{12}$, with American Seed No. 27, beyond 4 or 5 seconds are a mistake. Almost nothing can be gained in the extension, and the details are sure to suffer. Some of the old collodion preparations, on the other hand, resist this encroachment on the internal structure in a wonderful manner, and by relatively lengthening the exposure the same extension can be secured as when using gelatine plates. Much might be said to enforce this particular suggestion.

One more point should be carefully considered, and that is the size of the picture, or, as it may be specified, the diameter of the disk of the Moon on the plate. For any accurate measures a Moon one-third of an inch is very small, as the magnification should always be under the microscope of the dividing engine, and a picture of one and one-fourth inches, such as Prof. Pickering took at Willows, Cal., Jan. 1, 1889, is much more valuable. The mounting of a field apparatus of the required 12 to 15 feet focal length is quoted as an obstacle, but labour and money are not the real questions. Our American apparatus at Cape Ledo showed conclusively, so far as the mounting goes, that the triangle principle combined with a piston of flowing sand is perfectly available for operating even a forty-foot telescope, at a very slight expense of skill and money. The same plan is amply capable of setting up a telescope of any size from 10 to 25 feet focus with all the accuracy needed, and probably with as great precision of diurnal motion as any clock-work that would be carried to the station. A description of this apparatus is given in the government report of our expedition.

Unless I am greatly deceived by the results recently obtained in my study of the coronal streamers, it is the details of the structure of the corona that will be of permanent value in researches relating to solar physics, and therefore at least a portion of the preparations for another eclipse should be turned in this direction.

Frank H. Bigelow.

Motions of the Nebulæ in the Line of Sight.

In his very kind review of my paper on the Motions of the Planetary Nebulæ in the Line of Sight, Mr. Maunder calls attention to a number of doubtful but important points, of which recent observations enable me to speak with greater certainty now than at the time my paper was printed. In October the Orion Nebula again came within reach of the great telescope, and I was delighted to find that the third line in the third spectrum of the grating was sufficiently bright for an accurate comparison with terrestrial hydrogen, the probable error of one evening's determination of the displacement of the line being only 1½ miles per second. Such a determination consisted of 16 measures, and it was found unprofitable to reduce the probable error by making a greater number than this, as for some reason (by no means peculiar to this class of observations) the results obtained on different nights differed by more than the probable error of a single night's comparison. These observations not only give the motion of the Orion nebula by the displacement of a known spectral line, but they fix the true position of the brightest line, and hence allow definite numerical values to be assigned to the motions of the nebulæ included in my former list. Writing at such a distance from Mt. Hamilton, it is not possible for me to give the exact values obtained, nor, indeed, is it desirable, as the observations are not yet completed; but I may state provisionally that the comparisons indicated a motion of recession of the nebula from the Sun of some 9 or 10 miles a second. The motion deduced from a comparison of the position of the brightest line with the mean position assumed in my paper on the motions of the nebulæ agrees fairly with this, the difference being in the direction which would result from the situation of the ten nebulæ of my list in that quarter of the heavens towards which the solar system is known to be moving. In other words, the true position of the line, as determined by observations of the Orion nebula, is *lower* in the spectrum than the position previously assumed, a fact which Mr. Maunder pointed out as probable. The difference of position is not sufficiently great, however, to bring the line into coincidence with the terminal line of the magnesium fluting at λ 5006·4, the interval between the lines, small as it is, being much greater than the error of observation.

The motion of recession of the Orion nebula indicated by these observations may be wholly due to the drift of the solar system itself. The supposition seems plausible (although certainly no good reason could be given for it) that a nebula of such vast extent and extreme tenuity is more likely to be at rest relatively to the stars of our system than small, compact nebular masses or individual stars. If this is true the Orion nebula gives us a fixed reference point, and the motion of the solar system deduced from measurement of the displacement of lines in its spectrum is that which would be determined from observations of all the stars we see, the direction of the motion being assumed as known. Allowing for the distance of the nebula from the point opposite the vertex of the Sun's motion, the motion of the solar system given by the observations of the nebula of Orion agrees remarkably with the value assumed by Miss Clerke in her admirable essay on Rigel and the Great Nebula in the October number of the 'Observatory.'

Mr. Maunder's remarks on the greater brightness of images in small telescopes of relatively large aperture, as compared with the 36-inch refractor, are undoubtedly true, but the visibility of very small and faint objects depends so much on the size of the image (*i.e.*, in the case of a spectral line, its length), as well as on its intrinsic brilliancy, that the 36-inch refractor gives a distinct advantage in the observation of the planetary nebulæ. Thus, the intrinsic brilliancy of the third line in the spectrum of Σ 6 is greater than that of the same line in the spectrum of the nebula of Orion, but in the case of Σ 6 the comparisons with $H\beta$ were much the more difficult. The spectrum of the Orion nebula, however, would be more easily observed in a small telescope with the usual ratio of aperture to focal length.

The brightness of the nebular lines in the third and fourth spectra of such a grating as I have used is quite surprising; and although I have not actually made the experiment, it seems to me probable that the grating may give a brighter spectrum than a train of prisms having an equal dispersive power. The other advantages of a grating, such as lightness, ease of adjustment, &c., are too obvious to require mention.

Dr. Huggins has pointed out that my rather small value of the wave-length of the magnesium fluting (λ 5006·36) used for comparison with the brightest nebular line is probably due to the fact that my observations refer to the centre of the terminal line, while the measures of Watts and others (which give $\lambda = 5006{\cdot}5$) refer to its less refrangible edge. James E. Keeler.

Washington, D.C., Dec. 11, 1890.

Comet Perihelia.

In a communication* to the Vienna Academy of July last Dr. Holetschek has returned to the consideration of those conditions which increase or diminish the chances of cometary discovery, by discussing the connection between the heliocentric longitude of the perihelia of comets and the heliocentric longitude of the Earth at the time that the comets pass through their respective perihelia. This is a problem that he has previously treated in some detail in his paper on the direction of the major axes of cometary orbits; but two causes have contributed to destroy the accuracy of the conclusions to which that discussion led. One is that the solar longitudes were grouped roughly in periods of calendar months, and therefore each group did not contain an equal arc of longitude. The second is that the longitudes of the cometary perihelia were referred to the common equinox of 1850, and therefore the effect of precession in the case of ancient comets had been neglected.

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* Ueber den scheinbaren Zusammenhang der heliocentrischen Perihellänge mit der Perihelzeit der Kometen.

These two sources of uncertainty have induced Dr. Holetschek to abandon the method of grouping, and to draw up a table in which is exhibited for every known comet (omitting some suggested early returns of Halley's comet) the value of the arc

$$(l - L \pm 180^\circ),$$

where l and $L \pm 180^\circ$ are respectively the heliocentric longitudes of the comet's perihelion and that of the Earth. The contention is, that since a comet will be at its brightest when its proximity to the Earth occurs simultaneously with its approach to the Sun, and consequently most favourable for detection and observation, therefore in a majority of instances this arc $(l - L \pm 180^\circ)$ should be small. The case of comets with small perihelion distances (q) is, however, peculiar, since these comets cannot be seen at perihelion on account of their small elongation, but only as they approach to, or recede from, the Sun; and for those comets in which (q) is less than 0·30, the table exhibits the value of $(l - L)$, which, according to the theory here enunciated, ought also to be a small arc. The choice of the limit, $q < 0{\cdot}30$, corresponding as it does approximately with the perihelion of Mercury, is, of course, arbitrarily selected, and may operate adversely to the theory in the case of some comets when (q) lies between 0·3 and 0·5, even tending to produce a secondary maximum about 180°.

The study of this table is instructive. It is at once seen that the angular distances between the longitude of the Earth and that of the perihelia are most frequently less than ±60°, and that such distances as fall between ±120° and ±180° occur less frequently than between ±60° and ±120°, and so far support the theory which Dr. Holetschek advocates. Another feature is the uniformity with which the elliptic comets of short period conform to the rule. Though these comets appear in the table only at the times at which they have been accidentally discovered, yet, on account of their elliptic motion, they can present themselves at one of their many returns in a favourable position for detection, and would therefore unduly tend to swell the number of instances favourable to the hypothesis. These cases of periodical comets are therefore separated from the parabolic in the tabular arrangement which exhibits the number of comets whose perihelia fall within successive elliptic arcs of 30°, with the following result:—

$l-L\pm180°$.	Number of Parabolic Comets. $q>0·3$.	Number of Elliptic Comets.	$l-L$.	Number of Parabolic Comets. $q<0·3$.
° 0– 30 ..	90	16	° ° 0– 30	7
30– 60 ..	45	10	30– 60	12
60– 90 ..	46	3	60– 90	6
90–120 ..	26	2	90–120	4
120–150 ..	21	..	120–150	3
150–180 ..	22	..	150–180	1

It will be noticed that while the comets with perihelion distances greater than 0·3 conform to the rule with satisfactory regularity, in the smaller catalogue of those which can approach very closely to the Sun there is a tendency for the perihelia to group themselves about the arcs ±30°–60°, rather than about the smaller angular distances demanded by the rule. This discrepancy Dr. Holetschek thinks may be explained by the increased molecular action to which such comets are submitted by their close approach to the Sun. If this be the true explanation, we should expect to find that the mean perihelion distance of the comets forming the second of the groups is decidedly less than in the first group. But no attempt has been made by the author to establish this fact.

Several interesting statistical results are drawn from the figures of the general table, but they can only be referred to here very briefly. For instance, it is curious to notice that the positive or negative sign before the angle $l-L\pm180°$ occurs with comparatively equal frequency, not only in the total number of comets, but when grouped in shorter periods.

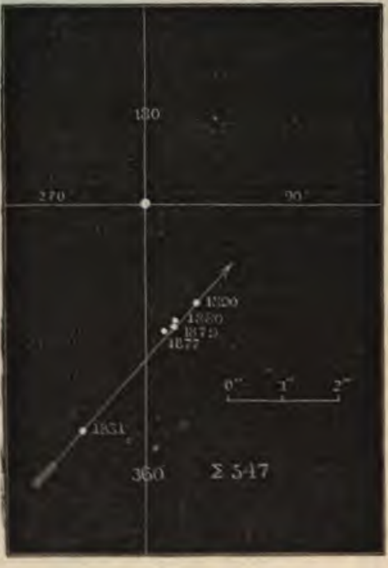
$l-L\pm180°$.	No. of Comets previous to 1759.	No. of Comets 1759–1835.	No. of Comets 1835–1864.	No. of Comets 1865–1890.	Total.
+	32	36	34	31	133
–	32	39	39	38	148

No conclusion can be drawn from these figures as to whether comets are more frequently seen in the morning or the evening sky, but it shows that the perihelion points, reduced to the ecliptic, lie, with equal frequency, in advance of and behind the Earth.

Each of these smaller groups is discussed in its bearing on the main question as to the frequency with which the arc ($l - L \pm 180°$) occurs as a small angle, and the same law, which obtains for the whole catalogue of comets, holds in the smaller groups. But if the group 1865–1860 be separated into two, that between 1865–1880, including 31 comets, offers an unexpected result by deviating from the general rule. Some ingenuity is exhibited to explain this abnormal deviation, such as the frequent occurrence in that period of comets with large inclinations. But such suggestions only shift the difficulty, without explaining it. No physical cause can be assigned why comets with orbits greatly inclined to the ecliptic should be more frequently met with at one period than at another. W. E. Plummer.

The Proper Motion of Σ 547.

It now appears beyond question that the change in this pair of small stars is due to proper motion. I have lately made a set of measures with the large refractor, which, in connection with the earlier observations, gives with substantial accuracy the value and direction of the movement. The following are the complete observations of this pair:—



1831·39......	344°·3	4″·25	8·5	11·5	Σ 3*n*.
1877·99......	9 ·8	2 ·31	..	..	β 3*n*.
1879·05......	14 ·0	2 ·26	9·0	12·2	β 4*n*.
1880·60......	16 ·1	2 ·19	9·0	11·7	β 5*n*.
1890·82......	28 ·5	2 ·01	8·5	9·7	β 3*n*.

I do not find any record of this pair having been seen between 1831 and 1877, although it was looked for by Dembowski in 1865, and by me, with the 6-inch instrument, a number of times previous to the date of my first measures with the 18½-inch. It should not be difficult with a small aperture if the foregoing magnitudes are correct. Variability of the smaller star has been suggested as an explanation of the failures to see it, but the proof on this point is not satisfactory. It is a small star, with other and brighter stars near it, and might have escaped examination when looked for. Argelander gave the magnitude of the larger star as 9·2, while Lamont called it 10.

The accompanying diagram shows the relative positions of the two stars from the measures given above. From this it appears that the apparent motion of the star is 0″·052 annually, in the direction of 137°·8. It is of course impossible to say from the measures to which component this proper motion belongs, but probably to the larger, which would then be moving in the direction of 317°·8. The minimum distance between the stars of 1″·9 will be reached in 1903. S. W. BURNHAM.

Selenographical Notes.

VIETA.—This beautiful object, the third in the fine chain of ring-plains which, beginning with Mersenius and ending with Piazzi, extends from the eastern border of the Mare Humorum to the region west of Bouvard, were it not much foreshortened by its position near the limb, would exceed in interest most other formations of its type and size. Even under the most adverse conditions of libration it will be found well worthy of examination under a low Sun, both on account of the remarkable character of the walls which surround it and the variety of detail in its neighbourhood; while when placed at its best, the curious and, as yet, doubtful nature of many of the features in the interior render it a specially interesting object for scrutiny. The most suitable time for observing Vieta under morning illumination is when the east long. of the terminator is about 65°. The greater part of the floor is then in sunshine, and there is still sufficient shadow associated with many objects upon it to render them tolerably conspicuous. At a somewhat earlier stage the prominent shadows of peaks ε and δ (see Neison, map xv.) on the lofty west wall, the first nearly 11,000 and the last more than 14,500 above the interior, are very noteworthy, but, of course, obscure many features on the floor. The shape of Vieta, like that of many other lunar formations, resembles an irregular hexagon, with long and nearly straight sides on the west and east, and with two much shorter sides on the south and north. The north-east and north-west sides are about equal in length and are slightly convex outwards, while the south-west side bends inwards and the south-east side is nearly straight. South of the lofty peak ϵ and north of the still more towering height δ, the wall diminishes very suddenly in altitude, and both on the south and north exhibits many breaks, being traversed in several places by narrow passes running down to the floor from the plain outside. On a low section of the south-western border stands a large deep crater, which is at least double the size shown by Mädler and Schmidt. Careful drawings of this part of the formation under various solar altitudes would be valuable, as there appear to be indications of the wall having been considerably modified by the presence of the crater. The detail on the floor of Vieta is somewhat puzzling to make out at the best of times, and this possibly accounts for notable discrepancies in published drawings and descriptions. Neison speaks of a "moderately-sized central peak with a small hill on each side and a craterlet on the east." Mädler shows two hills with a crater on the east in a line with them. Schmidt draws eight craters of various dimensions, but all of a small type, standing on and amid hills on the northern half of the interior, and a ridge on the southern section, which apparently does not answer to either of the three rills [S. 257, 258, 259] discovered by him in 1854, and depicted in Neison's 'Moon,' map xv., only one of which I have hitherto detected.

November 12, 1883, $8^h\ 30^m$ to $9^h\ 0^m$, and November 4, 1890, 8^h to 9^h, were two occasions on which I obtained especially favourable views of the formation. On the first, seven craters were easily visible on the northern quarter of the floor. Five of these were in a row, corresponding in position with Mädler's two hills and a crater. The third in the row, counting from the west wall, appeared to consist of two inosculating craters. The two remaining craters were at some distance to the north of the row and were much smaller. On the second occasion (last month), though definition was good and the same instrument and powers (280 and 340, on a 8½ S. O. G.) were used, I could, with all care, detect only the most easterly and westerly craters of the row, with an intermediate hill (? Mädler's central mountain α) and a third smaller crater north of these, about midway between the west and the east walls, and another low hill east of this. The two craters pertaining to the row were filled with shadow, and certainly seemed to be larger and more distinct than anything of the kind noted on the floor during the previous observation. A considerable difference in the condition of libration on the two occasions may possibly account for the fact that a greater number of craters were revealed on November 12, 1883, than on November 4, 1890, though the illumination was practically the same on both nights, but it does not explain why there should be any difference in the apparent size and distinctness of the two end craters of the row. With so many strange instances of discordance as regards observations of detail made under similar conditions, it seems to me that the only way of accounting for them is on the supposition of the existence of some obscuring vapour which occasionally either veils rills, small craters, &c, &c., from view altogether, or interferes considerably with their visibility. Vieta A and B, two notable twin ring-plains on the east of Vieta, were close to the terminator at 8ʰ on November 4, 1890. On the wall common to both stands a bright crater, shown only by Schmidt, and there appeared to be a break south of this on the same wall which seems to represent a second crater. There is a curious straight ridge running in a northerly direction from the north side of B, parallel to the east border of Vieta, and terminating in a small crater, which latter is connected by a doubtful narrow dark line (? cleft) with a large, irregularly-shaped depression under the north-east wall of Vieta, and can be traced down its inner slope. This region will repay careful examination, as it abounds in unrecorded detail, and includes some clefts not catalogued. The beautiful group of craters north-west of Fourier is very conspicuous at this phase. One member of the group (*d*) is remarkable for the brilliant whiteness of its inner slope, while its fellows, with two or three exceptions, are comparatively dark. Can lunar theorists explain why this should be?

THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1890, Dec. 18.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Sun-spots in High Latitudes.

GENTLEMEN,—

The past summer and autumn have brought us a decided increase in the number of spot-groups to be seen on the Sun, so that whilst there were but 32 groups recorded in 1889, 42 were observed in the first nine months of the current year. And with the quickening activity of the Sun, the tendency of the spots to seek high latitudes has been increasingly distinct. As in 1889, what we might call mid-latitudes on either side of the equator were absolutely free from spots. These barren zones were not quite so wide as in 1889, extending only from 11° to 18° instead of from 10° almost to 20°, but the avoidance in either hemisphere of this belt, which at maximum, and on the average for an entire cycle, is by far the most frequented of any, was very conspicuous. The equatorial zone, too, was almost deserted. Only five groups out of the 42 were situated near the equator, and of these three were only seen on a single day, and one group was a legacy from 1889. One considerable group alone, belonging wholly to 1890, was found in the equatorial zone, the group which ran its course from July 22 to August 1.

The remaining 37 groups all had latitudes of 19° or over, 17 being northern and 20 being southern groups. Four groups had a latitude of more than 30°, two of these being groups to which I have already referred in your pages *, and the other two being but small groups.

The principal groups of the year so far have been as follows, neglecting the group which was first seen on 1889 Dec. 27 and which passed out of sight at the west limb on Jan. 7 in the present year:—

	Lat.
Jan. 17–22	+23°
March 4–14	+33
May 8–13	−28
June 5–9	−25
July 4–11	−24
July 7–14	−21
July 22–Aug. 1........	− 7
July 28–Aug. 6........	+19
Aug. 25–Sept. 6	+20
Sept. 1–9	−19
Sept. 15–18	−20
Sept. 17–20	−27
Sept. 24–Oct. 2........	+21

Two fine groups have been seen on the Sun since the beginning of October, but unfavourable weather prevented either being fully observed at Greenwich. The first, a particularly fine and interesting group, would appear to have reached the east limb on Oct. 19 and the west limb on Oct. 31, its latitude being about −24°. The other lay in the northern hemisphere, latitude +19°, and was visible during the last ten days of November and the beginning of December. A third but smaller group was observed in latitude −28° in the middle of December.

Yours faithfully,

Blackheath, 1890, Dec. 22. E. W. MAUNDER.

Conjunctions of Jupiter's Satellites.

GENTLEMEN,—

The following are the Greenwich Mean Times of the visible Conjunctions of Jupiter's Satellites, relative to Jupiter's equator, as seen from the Earth in January 1891. The satellite nearest the Earth is named first. Apparent distances from the centre of Jupiter are given in multiples of his equatorial radius. E. and W. mark the positions of the Satellites relative to Jupiter. * shows that the planet is above the horizon of Greenwich; † that the satellites are moving in the same direction with relatively slow motion; ‡ denotes a very close approach to a conjunction.

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* See the 'Observatory' for April 1890, p. 159, and for July, p. 241. May I take this opportunity of correcting a clerical error in the latter place? The word "March" should be "May" throughout the entire letter.

d	h	m				d	h	m			
1	16	2	III. II.	9·4	E.*	11	1	55	I. II.	5·6	W.†*
2	12	47	III. I.	1·25	W.	11	18	43	I. II.	5·6	E.†
2	16	32	III. IV.	3·2	W.	12	11	14	IV. III.	9·4	W.
2	17	19	II. IV.	3·0	W.	12	18	23	I. III.	5·9	W.
3	0	2	III. II.	7·0	W.‡*	13	16	38	I. III.	5·8	E.
4	14	29	I. II.	4·5	E.‡	15	9	0	I. II.	5·77	E.†
5	14	15	I. III.	5·6	W.	16	14	58	II. III.	2·5	E.†
6	9	9	I. III.	4·5	E.‡	17	13	57	III. II.	9·4	W†.
8	0	22	I. II.	2·25	E.†*	18	10	40	III. IV.	14·8	W.
8	20	45	III. II.	9·2	E.†	18	10	51	I. II.	1·7	W.†
9	18	51	III. II.	9·1	W.†						

As so few of these phenomena are visible within the limits of the British Isles comprising 48^{m} only of Longitude from E. to W., I have endeavoured to secure ampler opportunities for observation by sending manuscript copies of this list to the observatories of Madras and Washington, distant 5^{h} 21^{m} East and 5^{h} 8^{m} West, respectively, by recent mails.

Faithfully yours,

A. Freeman.

Murston Rectory, Sittingbourne,
1890, Dec. 15.

The early History of the Red Spot on Jupiter.

Gentlemen,—

In connection with Mr. Ryle's letter in the December number of the 'Observatory,' I trust that I shall not be considered egotistical if I refer your correspondent to a paper by myself, read before the Royal Astronomical Society in 1887. It will be found on p. 515 of Vol. xlvii. of the R. A. S. 'Monthly Notices.'

Faithfully yours,

William Noble.

Forest Lodge, Maresfield, Uckfield,
1890, Dec. 5.

Periodical Comets due in 1891.

Gentlemen,—

Our old friend Encke's comet may be confidently expected to turn up again next autumn, as it has been seen at every return since Encke determined its period at the appearance in 1818-19, besides having been observed on three previous occasions, in 1786, 1795, and 1805. We have reason to be grateful to this comet for affording the means of the earliest determination of the mass of the planet Mercury; but it has other problems yet to solve. Encke's theory respecting the cause of the diminution of the length of its periodic time could not be held (at any rate in its original shape) when it was found that that diminution was not constant in amount. Mr. Monck, I believe, was the first to suggest the retardation produced by traversing or passing near meteoric streams as a cause, and on this we may hope that further light will soon be thrown.

Two other small comets are due to return to perihelion some time in the coming year. Both were first discovered by Tempel, but as the periodicity of one of these was not detected until after its re-discovery by Prof. Swift in 1880, it is usually called Swift's comet. Tempel discovered it in November 1869; it has a period of about 5½ years, but escaped observation in 1875. It was unfavourably placed at the return due in 1886 and was not seen; another return will be due in the autumn of 1891.

The comet which was discovered by Tempel in 1867 has a period of about six years. It was observed at returns in 1873 and 1879, but not at the last, which was due in 1885. Another return therefore will be expected in the course of the year on which we are just about to enter. Yours faithfully,

W. T. Lynn.

Blackheath, 1890, Dec. 18.

OBSERVATORIES.

Copenhagen.—M. Victor Nielsen kindly sends us the following letter:—"In my private observatory I hope shortly to mount a 7½-in. refractor by Reinfelder and Hertel of Munich (mounted by W. Falcz-Rasmuyssen of Copenhagen). My work has recently been chiefly selenographical; but with my 6½-in. I have photographed the Pleiades, 61 Cygni, the Orion nebula, and Brooks's comet (May, 1890). The comet was too faint to measure, and the stars were small lines owing to the cometary motion (exposure 35 min.). I hope with the new instrument to do better photographic work, for the actinic focus is only 1½ mm. from the visual, whereas in the 6½-in. it was 7 mm. distant. The O.G.'s of the new instrument and its finder are of Jena glass. I beg to thank the obervatories which have sent me their publications: viz., Lick, Washington, Warner, Markree, Rousdon, Meudon, Prague, Baron Engelhardt's, O'Gyalla, Vienna, &c."

Moscow.—A volume of the Annals, dated 1890 and numbered "II. Série, Volume II., Livraisons 1 et 2," chiefly consists of various papers on comets and meteors and kindred subjects, by the Director and his staff. M. Bredichin's name is already famous in this connection, and what he writes will be read with interest.

One chapter only is observational, being an account of pendulum operations by MM. Sternberg and Bredichin, with a reversible pendulum, at eight stations, including Moscow. The largest discrepancy between two determinations at the same place is $0^{\mathrm{mm}}\cdot035$; and there appears to be a systematic error of $0^{\mathrm{mm}}\cdot240$, which is ascribed to vibration of the supports. A simple photometric apparatus and a chart for meteor observations are described by M. Ceraski.

Nice.—We have examined the third volume of the Annals of this Observatory with a feeling of simple reverence. It contains the magnificent map of the solar spectrum which M. Thollon scarcely lived to complete, and which is now a monument to his memory. The prismatic spectrum is mapped from A to *b* on such a scale that the distance between the D lines is 30 mm.; and for the four cases of a high sun; a sun of Z. D. 60° with dry air; Z. D. 60° and saturated air; and Z. D. 80°. No attempt has been made to reduce the positions of the lines to wave-lengths, but tables accompanying the map give in figures the exact positions (to $0^{\mathrm{mm}}\cdot01$), the intensities, and the breadths of the various lines. Of the labour involved in such a work, there are very few competent to form an opinion, and still fewer who can appreciate its excellence. The author compares it to a table of logarithms, as being a laboriously compiled work of reference; but, if we may gather from this that he feels the same confidence in the accuracy of his work as though it were a matter of simple calculation, his claims to our consideration are on a very different footing from those of the computer.

There are other matters of interest in the volume. M. Perrotin publishes part of a Theory of Vesta on which he has been at work. There follow the meridian observations 1887 March to 1888 March; then observations of nine comets and sixty small planets with the 15-inch equatorial, and finally elements of planets discovered at the Observatory. The liberality of M. Bischoffsheim is certainly being amply rewarded.

Yale.—The Report of the Secretary for the year 1889–90 shows that no change has been made in the general working of the observatory. Dr. Elkin's report on the heliometer work is appended, and we learn that 54 series of measures of Victoria and 33 of Sappho were secured. The reductions have been kept well forward, while considerable progress has been made towards the deduction of the solar parallax from the observations of Iris in 1888. Mr. Chase will carry out the heliometric triangulation of the Victoria comparison stars as suggested by Dr. Gill. Dr. Elkin is now completing his determination of parallaxes of first-magnitude stars in the northern heavens.

Lick.—Prof. Holden announces that the Edison General Electric Company have generously presented complete plant for lighting the observatory by electricity.

PUBLICATIONS.

Cordoba Observations.—Vol. XII. containing the zone observations made in 1879, under the direction of Dr. Gould, has been issued by his successor, M. Thome. The number of observations during the year was 19382 of 6213 stars. The separate results are given in ledger form, and the combined mean places in a catalogue. A few observations of Tempel's Comet are also given.

Rio Janeiro Annales.—Vol. IV. Part II. contains the Meteorological observations, 1883–1885, and as soon as possible those for 1886–1888 are to be printed. The gap in the series (from 1867–1881) will then be taken in hand. The volume before us gives details of observations taken every three hours, also ten day and monthly means of the same. We hope the Republic will extend to the observatory the same generous support that it received from the ex-Emperor.

The Story of the Heavens.—A new edition of Sir Robert Ball's popular work has been published, and we see no reason why many more editions should not follow it. The work is little more than a reprint, but with good reason ; for though important astronomical work is being done every year, it is such as can scarcely find a place in a work dealing mainly with fundamental facts. We notice that Mr. Roberts's photograph of the Andromeda nebula is included as an illustration, though nothing is said of its significance, perhaps through excess of caution.

It would appear that some 14,000 people have already bought this work, and the fifteenth thousand are already attacking the market. We congratulate the genial author and wish him continued success.

Astronomical Papers for the use of the American Ephemeris.—In Part 5 of Vol. II. Prof. Newcomb gives a re-discussion of the observations of the Transits of Venus 1761 and 1769. All observations have been subjected to a most searching criticism, and modern longitudes of the observing stations used. The alleged falsification of Fr. Hill's observations having been thoroughly disproved, great weight is attached to them. Numerous observations not used by Encke have been incorporated, and the observations have been corrected for the imperfection of telescopes and other matters which militated against distinct vision of the thread of light. From the two Transits $8''{\cdot}79 \pm 0''{\cdot}05$ is the resulting parallax, and $+32''\cdot4$ the correction to Le Verrier's longitude of the node of Venus. It will be remembered that Encke's result was $8''\cdot58$.—*Ast. Journal.*

Radcliffe Observations, 1886.—The 44th volume of Radcliffe observations has just been received. There are no changes to record in the staff or in the form of the volume. The annual catalogue contains 997 stars; the Sun and Moon were regularly observed, and the observations and resulting errors of N. A. are given. In the case of the Moon the errors are also corrected for the "Change in the Unit of Mean Time introduced in the year 1864." The volume also contains detailed meteorological observations.

NOTES.

The Recent and Previous Cold Periods.—The following particulars in regard to the recent and previous cold periods have been drawn up from the records of the Royal Observatory, Greenwich.

The month of October of the year 1890 was on the whole cold. In November there was a warm period, extending from November 12 to 24. Then on November 25 a cold period commenced, and, at the time of writing (December 27), still continues. Considering the period, November 25 to December 26, we have already one of 32 days, with a mean temperature of only 29°·9, or 11°·2 below the average; on 20 of these days the mean daily temperature was more than 10° below the average, on 5 of them being more than 15° below; from November 26 to 30, December 10 to 20, and December 22 to 25, it was continuously more than 10° below, the greatest departure from the average being −20°·7 on December 22. From December 10 to 19 the temperature was never above the freezing-point.

It becomes interesting to compare this record with previous instances of frost occurring before Christmas, adding thereto the character of the weather, as regards temperature, during the immediately succeeding part of each winter.

In 1853–54 a very cold period set in on December 11, lasting till January 6, or for 27 days: the mean temperature (m.t.) of the period was 30°·9, or 7°·1 below the average; on 5 days the mean daily temperature (m.d.t.) was more than 10° below the average, the greatest departure therefrom (g.d.) being −13°·2 on December 29. During the rest of January the temperature was generally considerably above the average.

In 1855–56 cold set in on November 13, lasting nearly continuously till December 22, =40 days: the m.t. of the period was 35°·6, or 5°·5 below the average; on 8 days the m.d.t. was more than 10° below the average, the g.d. being −18°·5 on December 21.

The rest of December was warm, and January, with a few exceptions, was also warm.

In 1870–71 the early part of December was cold and the middle portion warm. On December 21 cold set in, lasting, with a few exceptions, until February 2, =44 days: the m.t. of the period was 31°·2, or 5°·9 below the average; on 10 days the m.d.t. was more than 10° below the average, the g.d. being −17°·0 on December 25. The rest of February, with one or two exceptions, was warm.

In 1874–75 a cold period set in on December 9, lasting till January 1, =24 days: the m.t. of the period was 31°·1, or 7°·9 below the average; on 6 days the m.d.t. was more than 10° below the average, the g.d. being −16°·4 on December 31. Followed by a very warm January.

In 1878–79, after a somewhat cold period, one of greater cold set in on November 26, lasting till December 25, =30 days; the m.t. of the period was 32°·1, or 9°·1 below the average; on 12 days the m.d.t. was more than 10° below the average, being for 8 days, December 8 to 15, continuously more than that amount below the average, the g.d. being −17°·3 on December 24. Then, after 7 days of warm weather, there followed a very cold January.

In 1879–80, after a variable period, cold set in on November 20, lasting till December 27, =38 days: the m.t. of the period was 31°·3, or 9°·8 below the average; on 15 days the m.d.t. was more than 10° below the average, being for 8 days, November 30 to December 7, continuously more than that amount below the average, the g.d. being −19°·2 on December 7. Then, after 8 days warm, followed a very cold January. Great similarity with the previous winter.

A few notes on the winter of 1854–55 (the Crimean winter) may not be out of place. After a variable period, until the middle of January, cold set in on January 14, lasting till February 24, =42 days: the m.t. of the period was 28°·4, or 9°·4 below the average; on 18 days the m.d.t. was more than 10° below the average, on 5 of these days being more than 15° below, and on 10 days, February 13 to 22, was continuously more than 10° below, the g.d. being −18°·0 on February 18.

As regards absolute low temperature in the months of November and December last, the lowest recorded in November was 18°·3 on the afternoon of November 28 between 4^{h} and 5^{h} P.M., the lowest previously recorded in November since the year 1841 being 19°·4 on 1856 November 30. The lowest temperature in December was 13°·4 on December 22, temperatures lower than which have been recorded only three times in December since 1841: they occurred 1860, December 25=8°·0; 1870, December 25=9°·8; and 1878, December 25=12°·2. W. E.

There is a Greenwich proverb that it is "never clear when the snow lies;" and the following figures will show how far the recent severe weather has confirmed this dictum. They give the number of transit observations made during December. The constancy of the watch kept at Greenwich is perhaps too well known to need comment, and it may fairly be said that the following observations were all that were possible during the month.

Date.	No. of stars.		Planets.	Date.	No. of stars.		Planets.
	Equat.	Polar.			Equat.	Polar.	
Dec. 1......	10	2	⊙ & ♀	Dec. 15......	2	—	—
2......	2	—	♂	16......	—	—	—
3......	—	—	—	17......	2	1	—
4......	—	—	—	18......	1	—	—
5......	—	—	—	19......	—	—	—
6......	1	—	—	20......	—	—	—
7......	2	—	☽	21......	4	—	☽
8......	—	—	—	22......	1	1	⊙
9......	9	1	♆	23......	—	—	—
10......	—	—	—	24......	8	2	♆ & ☽
11......	2	—	—	25......	1	—	—
12......	—	—	⊙	26......	—	—	—
13......	10	1	⊙♂♆	27......	—	—	—
14......	3	—	⊙	28......	—	—	—

It is doubtful whether 100 observations will be secured in the month (counting double limbs), the average for December in recent years being considerably over 400.

THE CLIMATE OF GREENWICH.—Some months ago, Dr. Auwers published a research on the Sun's diameter (see Obs. No. 156, p. 445), wherein he incidentally noticed an apparent decrease in the average annual number of meridian observations of the Sun, which he attributed to a deterioration of the climate of Greenwich. His figures were as follows:—

Years.	No. Obs.	Years.	No. Obs.
1750–60........	160	1811–18........	154
1765–75........	138	1819–27........	152
1776–86........	140	1828–35........	138
1787–98........	148	1836–46........	119
1799–1810......	159	1847–80........	114

It will be noticed that though the first number is undoubtedly the largest, and the last the smallest, the second number is exactly repeated more than fifty years later, and it would appear that up to 1835 at least there was no certain change. Since then the observations of the Sun have been intermitted on Sundays, and proper allowance must thus be made. If Sundays are comparable with other days, about 19 observations would apparently be the proper allowance, and this would make the last figures 138 and 133 respectively. This assumption has been confirmed by the experience of the past year, for the Astronomer Royal made arrangements for the observation of the Sun on Sundays, commencing with 1890, Jan. 1, and there have been 20 observations of diameter on those days. The deterioration in climate would thus appear to be small but sensible.

THE CORDOBA DURCHMUSTERUNG.—In the Astr. Journal for Dec. 8, Mr. Thome, the Director of the Argentine National Observatory, gives an interesting account of the work of his Observatory in extending the Durchmusterung to −42°. "During the past five years our efforts have been devoted mainly to the extension of the Southern DM., from the limit to which it had been carried by Prof. Schönfeld; or rather from the beginning of −22°, so as to have that belt (one degree) in common with Schönfeld's great work, for comparison of results. Up to the present we have made more than a million of observations of objects situated in the belt between −22° and −42°, which have all been reduced to a common epoch (1875·0), and in the course of a few months the final revision for that entire region will have been completed . . . . The telescope employed is a small equatorial by Alvan Clark & Sons, of 12½ cm. aperture and 168 cm. focal length, with an eyepiece magnifying 15 times, showing stars to the 10½ magnitude, under the best conditions of sky in dark field." From the comparison with Schönfeld's work, it appears that nearly double the number of stars have been observed at Cordoba in the same region; though the probable error of an observation is rather large, owing to the use of an occulting bar instead of an illuminated reticule. The work cannot fail to be of great use, and it will be of great interest to compare the results with those of the Cape photographic DM.

MOTION OF THE SOLAR SYSTEM IN SPACE.—Herr Oscar Stumpe has made another computation of this motion, using 1054 stars, whose P.M. in arc is greater than 0″·16. These include the Bradley stars whose P.M. was determined by Auwers, and those determined by Argelander; the remainder come from miscellaneous sources, and represent much personal labour on the part of Herr Stumpe.

The author deals with the vexed question of the relative distances of the stars in a somewhat novel manner. He assumes that the distances are inversely proportional to the apparent proper motions, then dividing the whole number of stars into four groups according to the magnitude of their P.M.s he finds the angular coordinates of the apex of the Sun's way for each of the groups, and also the angular velocity of the motion as seen from the mean distance of the group. Now, if the assumption as to distribution be correct, the angular coordinates should, of course, be the same for both groups; but the angular velocity should vary inversely as the mean distance of the group, and therefore, ex hypothesi, directly as the mean P.M. This is found to be the case, as the following figures will show. In the third column is given the deduced angular velocity, and in the fourth the mean P.M.; the ratio of these in the fifth column is seen to be nearly constant:—

	No. of Stars.	A.R.	Dec.	Ang. vel.	Mean P.M.	Ratio.
Group I.	551	287°·4	+42°·0	0″·140	0″·23	1·6
„ II.	340	279 ·7	+40 ·5	0 ·295	0 ·43	1·4
„ III.	105	287 ·9	+31 ·9	0 ·608	0 ·85	1·4
„ IV.	58	285 ·2	+30 ·4	2 ·057	2 ·39	1·2

Herr Stumpe remarks that since the mean magnitude of each group is about the same (6·0, 6·7, 6·1, 6·5), it would appear that the P.M. is a much more satisfactory criterion for the distance of a star than its brightness.

The linear magnitude of the velocity of the Solar system is accordingly left undetermined. We must probably depend on spectroscopic results for this information.

Distant Comets.—The great Lick refractor has to reckon another achievement in opening up the possibilities of following comets for a much longer period than before. Mr. Barnard has been active in this direction, and hopes to follow the Comet 1889 I. for one or two years longer, while another, a faint Comet (1889 *b*), has already been followed for a year and a half.

The Refrangibility of the Light of Mars.—"Theoretically owing to the deficiency of blue rays in the light of Mars, it should suffer a less refraction than do the stars; and if the difference be sensible it has an important bearing on the solar parallax determinations from Mars." Accordingly Dr. Elkin has made some experiments on the subject with the Yale heliometer, but he finds that "it is not easy to draw any conclusion as to the relative refrangibility of the light of Mars and a star, beyond that the difference cannot be very large." (See 'Astr. Journal,' No. 229).

British Astronomical Association.—We are pleased to see that the B. A. A. is being taken up in the Colonies. The 'Cape Times' of Nov. 27 gives a summary of the steps leading up to, and an account of the first meeting. Mr. Cox, of the Observatory at the Cape, seems to be a S. African agent; if astronomers in other colonies will act in the same manner, we shall expect soon to hear of several colonial branches.

Tycho's Star.—Mr. Burnham has examined the region about the place given by D'Arrest for Tycho's star with the 36-inch. None of the faint stars near the place presented any peculiarity worthy of remark, but three double stars were found.

Double Stars.—The 'Transactions of the Royal Irish Academy,' vol. xxix. part 13, contains a collection of double-star measures made by Dr. Doberck during the period 1875–86. There are in all 7969 measures of position-angle and 2656 distances of 366 different couples. Most of these have already been published at different times, so that the value of the present Catalogue lies more in the compact form in which it is issued. The great majority of the measures were made with the Cauchoix equatorial at Markree, which has an aperture of 13·2 inches. The later measures were made at Hong Kong with the Lee equatorial. The tables in connection with the evaluation of the micrometer-screw are not very explicit, and form a great contrast with the catalogue itself, which is everything one could wish. The epoch of Σ 2114 (p. 415) ought to be 77·44.

AMERICAN OBSERVATORIES.—Mr. W. C. Winlock has written an interesting article on Observatories in the U. S. for an Encyclopædia, from which we glean that there are about 60 observatories, exclusive of those of amateurs possessing telescopes of 4 inches or less aperture. A list of observatories with their instruments, and special remarks, is given, and a brief history of Harvard and Lick.

The University of North Carolina appears to have been the astronomical pioneer of the States, for in 1824, Dr. Caldwell, the President, purchased in London the following instruments:—a transit and a zenith telescope (Simms), equatorial (Dollond), sextant (Wilkinson), reflecting circle (Harris), quadrant (Hadley), and clock (Molyneux). They were erected in the University Building and 7 years later the first observatory was built for their reception, and Caldwell, Elisha Mitchell, and James Phillips observed with them. In 1838 the building was partially destroyed by a fire, which seems also to have enveloped the records and the zeal of the observers, as it closed the astronomical history of the institution. The Hopkins Observatory of Williams College, which celebrated its jubilee a few years back, came next, having been established in 1836, and others followed at short intervals, until 1843, when Harvard was founded. It is worthy of note that in 1815 there was some idea of establishing an Observatory at Cambridge, but no action was taken until 1827, when the Dana house was purchased to serve as a temporary building, in which were mounted a 2¾-inch transit (Troughton and Simms), an equatorial of 5 feet focus (Short), and magnetical and meteorological instruments. Money accumulated, and the present site was purchased in 1841; but Harvard proper dates from the appearance of Donati's comet, which caused much excitement and sufficient enthusiasm to result in the sum of 25,730 dollars being subscribed to furnish Cambridge with a telescope of the largest size. Thus the foundation of the Harvard of to-day was laid. Of the history of Harvard it is needless to speak in the 'Observatory,' for its enterprise and energy are known to all our readers. The Lick Observatory is fully dealt with, but that, like the modern Harvard, needs no remark here.

One cannot but admire the interest taken in astronomy not only by the heads of colleges, but by the wealthy citizens of the United States, as evidenced by their establishment and endowment of observatories. Mr. Winlock's short article shows that America may be proud of her astronomical institutions, for it displays a record of rapid progress such as can be shown by no other country.

The same mail brought us the article on American Telescopes by Mr. Winlock, but this subject was referred to in No. 142, p. 394.

Comets Zona and Spitaler.—The former comet was discovered at Palermo by M. Zona on November 15, and the telegram conveying the news of the discovery reached Vienna about half-past two on the morning of November 17. M. Spitaler at once directed the 27-inch refractor to that part of the sky, and at the first glance saw a faint object in the field. Not being satisfied, as the object was much fainter than indicated in the telegram, he searched further and detected the comet Zona. On returning to the first object he found that it had moved in the interval. He was unable to see the comets again till Dec. 4. On Dec. 6 the faint comet (Spitaler) was observed at Copenhagen, and since then there are several observations, so that no doubt of its nature remains. The elements, however, of the two comets show no relation to each other, Zona having a parabolic and Spitaler an elliptic orbit.

Elements of comet Zona.

T = 1890 August 2·69291.

ω	328° 25′ 2″·1
☊	85 13 6 ·8
ι	154 7 12·9
log q	0·301557

Elements of comet Spitaler.

T = 1890 October 26·50833.

π	58° 24′ 28″·2
☊	45 7 51 ·2
ι	12 51 49 ·0
φ	28 11 26 ·6
log a	0·537532.

Period 6·4 years.

The Zodiacal Light.—Prof. C. M. Smith read to the R.S.E. on April 7, some interesting notes of his observations on the zodiacal light. He has observed the spectrum on many occasions for the last fifteen years (in India?), and for the last eight years with a specially designed spectroscope; and in all the observations the spectrum has appeared continuous and free from bright lines except during the spring of 1883, and "even then the lines were not seen with sufficient distinctness to make their existence certain." The most strongly suspected line was about W.L. 558, very near the auroral line at 556·7, which indeed was observed by Angström, in 1867, in the zodiacal light, though Prof. Smith is inclined to think it really belonged to the aurora, whose spectrum at Upsala can often be seen in almost all parts of the sky. Prof. Smith calls attention to the fact that more accurate information is needed as to the position of the axis of the light, the results of different observers being discordant.

Saturn and its Ring.—An interesting monograph on Saturn, the result of fourteen years' work, is contributed by Prof. Asaph Hall as Appendix II. to the Washington Observations, 1885. The characteristic of this memoir is great caution; and the three drawings of the planet, where a few scanty markings represent all that Prof. Hall can certainly see with a fine telescope, should reassure those who have been dissatisfied with their modest instruments because they could not therewith recognize the elaborate detail described by more imaginative observers. To quote the author's own words:—"The appearance of Saturn in our 26-inch refractor undergoes great changes from night to night, and sometimes even from hour to hour during the same night. Probably these changes are due to variations in our own atmosphere and in the action of the objective, and they do not therefore indicate real changes in the planet. Whenever we have a steady and transparent atmosphere, the outlines of the planet, the faint belts and markings on the Ball, the shadow of the Ball on the Ring, the dusky Ring, and the Cassini division are clear and distinct, and the abnormal phenomena sometimes seen are not visible. Without exception, my experience is that on good nights the planet always has this natural appearance. But on poor nights, when the image is blazing and unsteady, one can see and can imagine many strange things about this wonderful object."

Prof. Hall finds for the rotation period of the planet, from observations of the white spot (1876 Dec. 7 to 1877 Jan. 2), $10^h\ 14^m\ 23^s.8 \pm 2^s.3$ mean time (see Ast. Nach. No. 2146). Careful discussions are also given of the position and dimensions of the Ring.

The notch in the outline of the shadow was never seen at Washington, either by Prof. Hall or his assistant. "The curvature of the outline of the shadow presented an anomaly in 1876 when the convexity appeared to be turned towards the Ball, contrary to what we should expect from geometrical considerations. The notes show that something of this kind was seen after the reappearance of the Ring in 1878. . . . After the Ring was well opened, the curvature of the outline always appeared natural or turned away from the Ball."

Minor Planets.—The following have been named by M. Charlois:—No. 283, Emma; No. 284, Amelia; No. 285, Regina; No. 289, Nenetta; No. 293, Brasilia; No. 294, Felicia.

In 'Engineering' for 1890, Dec. 19, there is a description, with four excellent illustrations, of the photo-telescope for Melbourne, made by Sir H. Grubb, which may be taken as representative of all the English telescopes (Greenwich, the Cape, Oxford, Melbourne, and Sydney) which will take part in the international scheme for charting the heavens.

We notice that Dr. Watts's "Index of Spectra" has just been published. It is a work which should prove of the greatest use.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 172. FEBRUARY. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, January 9, 1891.

Lieut.-Gen. J. F. Tennant, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. Knobel and A. M. W. Downing, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Downing. Fifty-eight presents have been received since the last Meeting; among those calling for special mention are:—'The Scientific Papers of J. Clerk-Maxwell,' 2 vols., presented by the Clerk-Maxwell Memorial Committee; 'The History of Harvard College Observatory,' by D. W. Baker, presented by the Observatory; 'Manuscript Observations of Shooting-Stars, 1873–89,' by W. F. Denning, presented by the Author; a Bichromate Battery and Ruhmkorff Coil, presented by J. E. H. Peyton, Esq. The thanks of the Meeting were voted for the presents.

Mr. Knobel read a paper by *Mr. S. W. Burnham*, of the Lick Observatory, on the Proper Motion of H 1968. The faint star was observed and measured by Herschel in 1830 or 1831, the exact date of observation being unknown; but Mr. Burnham has made a series of measures of it during recent years, and finds a regular rate of movement, from which it seems probable that Herschel's observation was made in 1831. Argelander gave the proper motion of the star as $0''\cdot635$ per annum, but this has been shown by Struve to be an over-estimation. Mr. Burnham finds that this proper motion entirely fails to account for the motion of the small star that he has observed, and concludes either that the proper motion is in error, or the smaller star has a proper motion of its own. No orbital motion can account for the changes in position.

A vote of thanks was accorded for the paper.

Mr. Knobel read a paper by *Prof. H. C. Vogel*, of Potsdam, entitled "Remarks on Mr. Fowler's Note on the Duplicity of α Lyræ." Mr. Fowler would, indeed, have to be felicitated if, at the very outset of his work of photographing stellar spectra, he had succeeded in making a discovery of such great interest as the duplicity of α Lyræ; but it seemed to him (Prof. Vogel) that Mr. Fowler's conclusions could scarcely be accepted without independent confirmation. The spectrum of α Lyræ has been repeatedly photographed at Potsdam between Sept. 28, 1888, and Nov. 26, 1890, and in no case have the lines been found doubled in the manner shown in Mr. Fowler's photographs. The dates and exposures of the photographs taken at Potsdam are as follows:—

Sept. 28, 1888:	25	minutes	exposure.
Nov. 8, 1888, $8^h\ 0^m$ Potsdam M.T.:	60	„	„
Nov. 13, 1888, $7^h\ 15^m$ P.M.T.:	70	„	„
May 30, 1889, $10^h\ 30^m$ P.M.T.:	60	„	„
June 6, 1889, $10^h\ 15^m$ P.M.T.;	30	„	„
Sept. 15, 1889, $10^h\ 5^m$ P.M.T.:	30	„	„
Nov. 24, 1889, $6^h\ 40^m$ P.M.T.:	30	„	„
Nov. 25, 1890, $5^h\ 58^m$ P.M.T.:	45	„	„
Nov. 25, 1890, $10^h\ 45^m$ P.M.T.:	40	„	„
Nov. 26, 1890, $8^h\ 43^m$ P.M.T.:	45	„	„

It is easy to see that if α Lyræ is a double star of 24·68 hours period, as suggested by Mr. Fowler, the lines should be double in some of the photographs taken at Potsdam; yet although the definition is very good and the lines are very sharp, no doubling is found in any case. A relative motion of more than 20 miles a second could not fail to show itself on any photograph taken with the Potsdam spectrograph. With a period of 24·68 hours twice only during that interval will the relative motion be less than 20 miles per second, and that only for the very short time of ·43 hours, so that in the cases of 45 minutes exposure, as on Nov. 25 and 26, 1890, it should be absolutely impossible for the lines to be perfectly sharp; at the very least a relative motion of 50 miles per second should be shown. The Potsdam spectrograph photographs a region from λ 420 to λ 460, so that the K line, the duplicity of which first attracted Mr. Fowler's attention, is not included in the photographs taken with it, but the displacement, if really due to motion of the stars, would be as obvious in this region as in any other. Measurements of the positions of the lines at 452·30, 453·43, and 454·00 show that there is not any displacement greater than ±5 miles. With the lines of terrestrial hydrogen at 432·62 and 455·22 simultaneously photographed on the plate with the spectrum of Vega, no displacement can be detected. Even supposing these lines belong only to one star, then they should indicate velocities up to 185 miles per second. With β Aurigæ photographs obtained at Potsdam clearly indicate the periodical doubling of the lines, but this is never found in the case of Vega.

Special knowledge and great care are necessary to use the method of prisms in front of the object-glass adopted by Mr. Fowler, otherwise the arrangement is liable to produce duplication or even triplication of the lines in the spectrum of a star. At Potsdam it is practically almost impossible to get any double lines unless they are due to real motion of the components of the star, because a slit spectroscope is used. He (Prof. Vogel) would suggest that Mr. Fowler should thoroughly test his instrument on other stars, and should test his method of working by taking photographs of β Aurigæ. It would also be advantageous if he followed Prof. Pickering's example and tried the effect of using greater dispersion. Prof. Vogel also begged to call attention to the calculation and diagram given in Mr. Fowler's paper, as they fall far short of the accuracy that might justly be looked for in a paper of so much importance. Taking Nov. 1, 8 hours as the time of zero-point, and the period of 24·68 hours, a comparison of Mr. Fowler's observations with calculations shows the following results :—

Date.	Hour.	Calc. Motion.	Obsd. Motion.	Calc. – Obs.
Oct. 3......	9·00	313 miles	180 miles	+133
8......	8·00	314 ,,	370 ,,	– 56
9......	7·00	204 ,,	232 ,,	– 28
10......	7·33	176 ,,	260 ,,	– 84
11......	8·50	215 ,,	218 ,,	– 3
12......	8·25	139 ,,	294 ,,	–155
14......	10·00	172 ,,	260 ,,	– 88
17......	10·00	13 ,,	0 ,,	+ 13
27......	8·00	282 ,,	109 ,,	+173
28......	6·50	113 ,,	0 ,,	+113
Nov. 1......	8·00	0 ,,	0 ,,	0
1......	8·50	47 ,,	109 ,,	– 62
1......	10·00	180 ,,	180 ,,	0
4......	9·00	97 ,,	0 ,,	+ 97

An alteration of the zero-point ± half an hour, equal to about 50 miles, does not produce any improvement, and no reasonable accordance between calculation and observation, can be made out.

Mr. Fowler. In the face of the facts brought forward by Prof. Vogel, and the information I have received from Prof. Pickering, I feel bound to acknowledge that α Lyræ cannot be a double star as I suggested in November last. The facts with regard to my photographs, however, remain as I then stated them, and I have as yet no explanation to give of the phenomena. I have had enlargements made from some of the negatives, and I will pass them round with Prof. Pickering's photographs of β Aurigæ. When compared the similarity will be evident to all of you.

Since November I have not been able to obtain any photographs or to test the adjustments, mainly owing to the bad weather. The lines in the photographs shown are separated equally on opposite sides of the normal position. With one exception the lines were not double in the spectra of other stars which I have photographed with the same instruments. I have sent some of my negatives to Professor Pickering, and hope to get a satisfactory explanation from him.

Captain Noble. I think perhaps the meeting would also like to have some explanation of the extraordinary discrepancies in mere calculation. Mr. Knobel has read Prof. Vogel's results, and they differ widely from those given in Mr. Fowler's paper, which I have before me. I do not know whether they use Walkingham's Arithmetic at South Kensington, or whether they calculate according to Cocker there, but at all events the discrepancy is very great. The younger Fellows of the Society will notice that this paper is put forward as a mathematical demonstration of the duplicity of α Lyræ. The variations plotted in the astonishing diagram given are said to be represented by the curve of sines, with which they have really as much—or as little—to do as with the law of sky-signs, concerning which the London County Council has lately issued an edict. The whole paper professes to prove its conclusions on mathematical principles, and I think that Mr. Fowler should satisfy the meeting by showing on the blackboard or elsewhere the formulæ and mode of calculation by which he arrived at his quantities.

Mr. Fowler. I worked out my figures by the graphical method and laid no claim to great accuracy. The paper was only intended to be a preliminary one, and I hoped to get more results and to be able to work out the conditions more accurately. On account of the unfavourable circumstances of working in London, I was very anxious that the observations should be taken up elsewhere, and I am gratified to find that the matter has been taken up in Paris, Potsdam, and Harvard. It now remains for me to determine why the doubling of the lines is special to the instruments I employed.

Mr. Ranyard. I should like to ask Mr. Fowler how his prism is fastened in front of the object-glass. One frequently learns from one's mistakes. We are frequently near new discoveries when we have found something we do not understand. Is the prism clamped with screws, so that it may by any possibility be pinched? Such a pinch, if tight enough, would give rise to a strain in the glass which would cause it to behave like a crystalline structure and give double refraction.

Mr. Fowler. I do not think there is any possibility of pressure.

Mr. Ranyard. You have some photographs in which they are absolutely sharp, and some in which they are double?

Mr. Fowler. Very clearly double.

Mr. Ranyard. On going home after the meeting at which your paper was read I examined a photograph given me by the Brothers Henry, and in their photograph the lines were absolutely sharp and single. I have written to them since and they have taken three other photographs of the spectrum of Vega, and in each case have found the lines absolutely sharp and single. They use a large flint prism of 45° mounted on a board in a manner very similar to yours, and I can understand if there was any pressure the glass might be made doubly refracting.

Mr. Fowler. Yes; that seems possible. I only hope we shall learn something from the photographs, if it be only a source of error not suspected before.

Mr. Gregory. I might throw out a suggestion as to the probable cause of error in Mr. Fowler's photographs—one that I thought he would mention. At Kensington a lot of students use the Observatory, and they may have shifted the swing-back of the camera, and it is possible that this caused the duplicity of the lines. I saw Mr. Fowler investigating the photographs previous to his communication. He was as dubious about making it as any one could be, but in some of the photographs the lines were so sharp, and in others the doubling was so obvious, that the duplex character of the star seemed beyond doubt. It is very probable, however, that there is some source of error, and I think it is probably a movement of the swing-back of the camera.

Mr. Fowler. I may say that I never took a photograph without checking the position of the swing-back and of the focussing-screws.

Mr. Knobel. In this negative I fail to see any indication at all of duplicity.

Mr. Fowler. It is very difficult to see anything on the negative in gas-light except under the microscope, and considerable experience is necessary to make out the details.

A Fellow. I may make a suggestion that the doubling is due to halation—the reflection from the back surface of the glass plate.

The President. Then there would be two false lines—one on either side of the original line. The fine lines are said to be double here and the others are comparatively fuzzy, but they are broader. In any case halation would treble the line.

A vote of thanks was passed to Prof. Vogel for his paper.

Mr. Isaac Roberts then read a paper on "Photographic Evidence of Variability in the Nucleus of the Great Nebula in Andromeda." The paper was illustrated by means of the lantern.

The photographs may be briefly summarized as follows:—1885, August 30, exposure 30 minutes, the nebula showed no stellar nucleus, and the *nova* was near the centre; 1886, October 24, exposure 73 minutes, faint stellar nucleus; 1887, October 10, exposure 3 hours, very faint stellar nucleus; 1887, November 15, exposure 2 hours 35 minutes, no trace of stellar nucleus; 1888, October 1, exposure 2 hours, no stellar nucleus; 1888, October 2, exposure 2 hours 32 minutes, no stellar nucleus; 1888, December 29, exposure 4 hours, no stellar nucleus; 1890, October 12, exposure 3 hours 5 minutes, no stellar nucleus; 1890, November 1,

exposure 15 minutes, the nucleus is very strongly stellar; 1890, December 9, exposures 5 minutes, 15 minutes, and 60 minutes respectively, the nucleus is strongly stellar on each plate. There can be no doubt from these photographs that the nebula has a variable stellar nucleus, and it is shown that this variability can be studied by short-exposure photographs, say 15 minutes each.

Mr. Turner. I think one of the most satisfactory things to learn is that in the late inclement weather, during which most of us have been unable to do anything at all, Mr. Roberts in his new observatory at Crowborough has been able to take what he considers important photographs, and I am sure we can all congratulate him on those results.

Capt. Noble. In the majority of cases in which a stellar nucleus was visible the exposure was short, and the tendency of a short exposure is to produce an apparent stellar object and leave out the nebula.

Mr. Roberts. Pardon me; in one case of three hours exposure, and in another of one hour exposure, the stellar nuclei were shown.

Capt. Noble. Then my objection does not hold good.

The President. I would ask Mr. Roberts if he would give a description of his observatory.

Mr. Roberts. The observatory is placed on the summit of Crowborough Hill in Sussex, which is one of the highest points in the south of England, and commands the horizon around without material obstruction. The floor of the observatory is 780 ft. 7 in. above the sea-level. The whole of the buildings are erected on a level platform of concrete, the top of which is about 4 ft. above the ground, which slopes towards the S.E. The buildings are one story in height, and the floors are raised 15 in. above the platform or terrace, and are on one uniform level throughout. The limit to one story in height permits the telescope to be brought down to within 20° of the horizon when pointed over the roof of the house. The observatory is placed due south, and is 20 ft. square inside, with a transit-room opening from it on the west side. Adjoining the observatory on the north side is a physical laboratory and a chemical laboratory, with a dark room and photo-enlarging arrangements. These and the observatory are connected with the dwelling-house by a corridor, on one side of which is a mechanics' shop, heating chamber, and library. At the end of the corridor is the dwelling-house. The dome of the observatory is hemispherical and constructed with wood ribs, sheeted with wood and covered with copper. The dome has two slits each 3 ft. 8 in. in width, parallel with each other, and 24 in. between them. The shutters of the lower half of each slit slide horizontally round the dome, and the upper half slides upon and over the top of the dome, so that both slits can be opened the full breadth from the horizontal to 18 in. beyond the zenith. By opening both slits the observatory is soon cooled down to the external temperature. But there is one disadvantage in this dual-slit plan, for it gives the dome a pole which in some positions of the telescope is a little troublesome. The Latitude of the observatory is 51° 3′ 7″ N.; Longitude $0^h\ 0^m\ 37^s$ E. The site of the observatory was selected after much enquiry and investigation as to the probability of the occurrence of numerous clear intervals of sky during the year, suitable for the pursuit of stellar photography, and although it is premature to express a decided opinion, I am quite safe in stating that this locality is an improvement upon the former site of my observatory at Maghull. It may interest some to know that I commenced the buildings, the observatory and house, in the month of September 1889, and by September 1890 the buildings were finished and occupied, and some ten tons weight of telescopes, apparatus, and books was dismounted, conveyed, and re-erected on Crowborough Hill within twelve months' time. The photographic work was continued at Maghull till the month of June 1890, and resumed at Crowborough in the following October.

The President. I have been told that Mr. Roberts's Observatory is a pattern. It is full of all sorts of dodges and arrangements which are not only ingenious but admirably adapted for their purposes. I am only doing my duty to astronomy if I ask him to give us a much more detailed account of it, so that we may understand all the arrangements he has made, and when opportunity occurs we may take advantage of them.

Capt. Noble. I can endorse every word you have said as to the perfection of the arrangements. I never saw anything more beautiful in my life. There is everything that can be of assistance to astronomers, and one can only hope that the work in which he has so eminently distinguished himself at Maghull will be improved upon in his new quarters.

The President. He seems to me to be most fortunate in his new quarters, and I hope he will give us a fuller account of them.

Mr. Roberts. The best thing I can say is it will give me great pleasure to see any of the Fellows at Crowborough when the weather is fit.

Mr. Knobel. I hope Mr. Roberts will consent to the President's suggestion and give us a more elaborate description of his observatory; and if so, perhaps we might defer the publication of the description until we have drawings, &c.

The President. We will consider what Mr. Roberts has told us as a sort of whet to our appetite, and consider he is going to give us something more by-and-by.

A vote of thanks was passed to Mr. Roberts for his papers.

Mr. Turner then read a paper on the Determination of the Longitude Paris — Greenwich. He said that in the year 1887 a request was made to the Astronomer-Royal by the officers of the French Geodetic Survey that the longitude of Paris and Greenwich might be redetermined. It had of course been determined before—as, for instance, early in the century by rocket signals. As soon as the electric telegraph was invented, this longitude formed the subject of one of the first experiments on the determination of longitudes by that means in 1854; and the result, obtained by the eye and ear method throughout, was $9^{m}\ 20^{s}\cdot 51$, or a change of one whole second of time. Sir George Airy received a letter from Sir John Herschel asking whether this large correction was to be accepted, and he replied that the accordance of the results was so good as to justify him in accepting the new result as almost correct. But since then some doubt had been thrown upon the correctness of this value. In 1872 the officers of the U.S. Coast Survey made a direct determination which gave a result $9^{h}\ 21^{s}\cdot 0$, nearly $0^{s}\cdot 5$ different from that obtained in 1854. Again, there were various indirect determinations, as those *viâ* Vienna, *viâ* Bonn, *viâ* Berlin, &c. There was, indeed, considerable divergence of opinion about the proper longitude for Paris. It was not a question of some hundredths of a second, but of two or three tenths at least. The Astronomer-Royal very promptly entered into correspondence upon the subject, and it was arranged that the longitude should be redetermined, and that the parties to the redetermination should be the Greenwich observers and the officers of the French Geodetic Survey. The Paris Observatory could not take part in it. It was arranged that four observers, two French and two English, should share in the work, the observational portion of which was completed in 1888. The French observers were Colonel Bassot and Commandant Defforges, who had had considerable experience in longitude work in Algeria; the English observers were Mr. Lewis and himself (Mr. Turner). The computations were just completed. The preliminary discussion showed a wide divergence of nearly four tenths of a second between the results obtained independently by the two nations. It was thought at first there must be some mistake in the computations. The English and French computations had been made entirely independently. No mistake was, however, discovered after careful scrutiny, and it was then arranged that one of the French Observers should come to England and discuss the results in concert with the English Observers. In June last this was done, and they again went through the whole of the computations and could not find any mistake. It was subsequently found that the time determination at Paris and Greenwich differed by a very large amount, far more than could be explained by the personal equation of the two observers, which was circumstantially known. On further scrutiny it was found that the levels of the French instrument were somewhat suspicious, the results given by the nadir observation differing systematically by [illegible] from those given by the striding level, but as there was no evidence to show that the nadir method was more correct than the striding level, the mean of the two results was adopted. The English final result was $9^{m}\ 20^{s}\cdot 84$, but the French determination was $9^{m}\ 21^{s}\cdot 04$, the final difference between the two determinations being fully $0^{s}\cdot 20$. In making these longitude-determinations, the English and French observers were changed from station to station. Were the French nadir observations assumed correct, and the striding-level determinations in some unexplained way erroneous, the difference between the two results would be reduced to $0^{s}\cdot 04$. The general result of the whole series of observations was that it is not nearly so easy to determine the longitude as might be thought. It might be that the longitude was apparently variable like the latitude, as they had recently heard asseverated. There had been practically no independent check up to the present time on even fundamental longitudes, such as could be obtained by making two independent determinations at the same time. The only other instance in which two independent determinations were made at the same time also resulted in a discordance between the two. He insisted very strongly upon the necessity of more attention being given to this matter, which might so easily be neglected now that attention was concentrated on the development of physical astronomy and photography.

The President. We regret there has been a fiasco with regard to the determination of the longitude of Paris and Greenwich, but we hope some day it will be settled.

The Astronomer-Royal. I might mention that we are hoping in the present year to make a determination of the longitude of Montreal, and the experience we have had in this matter will be very useful in that operation. After we have completed that determination, we shall be able, I hope, to take up again the redetermination of the longitude of Paris. It has been a very disquieting thing that there should be this great discordance. The work has been carried out with all the precautions that could be suggested by the long experience of the French observers, who have had a great deal of experience, both in Algeria and in various parts of France, and every appliance was used that could be suggested with a view of securing increased accuracy, but, notwithstanding that, we have this very large discordance in two independent determinations carried on concurrently.

The thanks of the Meeting were accorded to Mr. Turner for his paper.

The Astronomer-Royal. There are two papers which are usually sent from the Royal Observatory to the Society at this time of the year. The first is the results of observations of the occultation of stars by the Moon and the phenomena of Jupiter's satellites. In consequence of the low altitude of Jupiter during this year, there are more discordances in the observations of the phenomena than would have been expected. We have endeavoured, whenever it is possible, to get observations by two observers of the phenomena of Jupiter's satellites, and in some cases there are very curious discordances, which I think will indicate to some Fellows of the Society who criticise the predictions of phenomena of Jupiter's satellites, that those observations are liable to a considerable amount of uncertainty. The other paper is on spectroscopic results for the motions of stars in the line of sight, and the results generally show accordance with the previous determinations. There are, as usual, certain discrepancies which may be due to some remarkable orbital motion in stars, such as have been indicated recently, or they may be due to errors of observation. I should not like to pronounce any definite opinion upon this point. In particular cases I think there is a mixture of two causes of discordance. With reference to these, we hope in a short time to have the 28-inch refractor substituted for the 13-inch, and then we shall be able to carry on these observations more effectively and get more accordant results. I have had a letter from Sir Howard Grubb to say that he has so far figured the 28-inch object-glass that the figuring appears to be perfect so far as 26 inches, and he hopes to be able very soon to get the outside inch of the circumference into good figure; and I think we may look forward to having that object-glass mounted and at work in the present year, and then I trust we may be able to get more satisfactory results.

A vote of thanks was passed for the papers.

Mr. Knobel read a short note from a paper by *Mr. Marth* entitled "A list of published Lunar Sketches and Photographs arranged according to the Sun's position."

A vote of thanks was passed to Mr. Marth for his paper.

The following papers were announced:—

Prof. H. C. Vogel. "Remarks on Mr. Fowler's note on the duplicity of α Lyræ."

A. A. Rambaut. "Observations on the Planets Victoria and Sappho made with the Meridian Circle at Dunsink."

S. W. Burnham. "The Proper Motion of H 1968."

Isaac Roberts. "Photographic evidence of Variability in the Nucleus of the Great Nebula in Andromeda."

Isaac Roberts. "Mr. Roberts's new Observatory on Crowborough Hill, Sussex."

A. Marth. "A List of published Lunar Sketches and Photographs arranged according to the Sun's position."

Royal Observatory, Greenwich. "Spectroscopic Observations of the Motions of Stars in the Line of Sight made during the Year 1890."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made during the Year 1890."

H. H. Turner. "Note on the recent determination of the Longitude of Paris."

E. J. Stone. "A comparison between the Greenwich Observations, 1887, and Hansen's Tables as corrected by Prof. Newcomb and Mr. Stone."

Professor Bhuban Mohan Bandyopadhyay, B.A., F.R.Hist.Soc Christian College, Lucknow, India; *Joseph Brooks*, Trigonometric

Survey of New South Wales, Lands Department, Sydney; *James S. Browne*, 170 Sloane Street, S.W.; *Edward Tremlett Carter, Assoc. Inst. E.E.*, 12 Princes Street, Hanover Square, W.; *Rev. A. L. Cortie, S.J.*, St. Bueno's College, St. Asaph, North Wales; *William Bladen Croft, M.A.*, 9 College Street, Winchester; *James Gallagher*, The Limes, Leyton, and St. George's House, Eastcheap, E.C.; *Rev. H. Gratton Guiness, D.D., F.R.Hist.Soc., F.R.G.S.*, Harley House, Bow, and Curbar, Derbyshire; *C. W. H. Jeavons*, Horseley Place, Tipton, Staffordshire; *John Mitchell*, Brockholes, Huddersfield; *William M. Pierson*, San Franscisco, California, U.S.A.; *James Lidderdale Scott*, Shanghai, China; *Arthur Laidlaw Selby, M.A.*, University College of South Wales and Monmouthshire, Cardiff; *Rev. Walter Sidgreaves, S.J., Director of Stonyhurst College Observatory*, Lancashire; *John Peter George Smith*, Sweyney Cliff, Coalport, Shropshire; *Arthur J. Walker*, New College, Oxford, were duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Thomas Richard Clapham, Gentleman, Austwick Hall, near Settle, Yorkshire (proposed by Herbert Sadler); *Arthur Kennedy, Solicitor*, 1 Clement's Inn, W.C. (proposed by J. E. Drower).

BRITISH ASTRONOMICAL ASSOCIATION.

The second ordinary Meeting of the above Association was held in Barnard's Inn Hall, Holborn, on Wednesday, Dec. 31, Captain Noble, F.R.A.S., the President, in the Chair.

Among the presentations announced was a beautifully executed diffraction-grating from Mr. John A. Brashear, of Allegheny, Pa., U.S.A., bearing a suitable inscription.

The General Secretary (Mr. P. F. Duke, F.R.A.S.) announced that 162 new Members had joined since the last Meeting, making a total Membership of 489. Several additional names were handed in during the Meeting, this being the last day for the enrolment of Original Members.

Mr. Franks read a first report of his Section, that of Coloured Stars. Thirty-five stars in Cassiopeia and Camelus had been observed; about 45 observations on the average had been obtained of each star. A number of comparison estimates of Polaris had been made for the purpose of obtaining the "colour equation" of each observer, but unfortunately it had been found that variations of Polaris appeared to be less between different observers on the same night than by the same observer on different nights.

Mr. Waugh's report of the Jupiter Section was read by Mr. Cottam (Secretary to the Sections). The report contained, amongst other interesting matter, an account by Mr. A. Stanley

Williams of a dark transit of Satellite No. 1 on July 26, when the shadow of the satellite appeared double.

The President remarked that the observers at the Lick Observatory thought that they had observed this satellite double or else that it was crossed by a very dark band.

Mr. Sadler believed that Sir James South had observed duplicate shadows of No. 1 Satellite.

Mr. Green then delivered a lecture on the Canals of Mars, illustrated by lantern reproductions of drawings by Schiaparelli, Boeddicker, Maunder, and himself. Mr. Green doubted the accuracy of Schiaparelli's canal-like markings, and pointed out that it was one thing to be able to see an object and another to be able to draw it correctly and artistically. He illustrated his point by presenting lantern-views of three of Schiaparelli's drawings of the Kaiser Sea made respectively in 1877, 1879, and 1882, and for comparison with them drawings of the same object made in the same years by Boeddicker, Maunder, and himself, and pointed out that, contrary to what might have been reasonably expected, the three drawings by the three different observers resembled each other much more closely than the three by Schiaparelli himself. The same point, he said, could have been established from the drawings of De la Rue, Lockyer, Trouvelot, Terby, Knobel, Neisten, and Kaiser, all of whom had drawn this region practically in the same way. He concluded by repeating that his criticism of Schiaparelli applied, not to his competency as an observer, but to his skill as a draughtsman and an artist.

The President remarked that he had never observed anything at all resembling Schiaparelli's canals on Mars.

Mr. Sadler urged that Schiaparelli was a very gifted astronomer, and in particular a first-class double-star observer, and suggested that possibly atmospheric changes, such as clouds and snowstorms, might account for the altered appearances.

Mr. Lawson remarked that geological changes of seas and continents might be taking place on Mars as they had done on our Earth.

Mr. Maunder (in reply to a question) stated that he had observed a few of the canals as delicate faint shaded lines, but never double; he added that the Kaiser Sea was indicated in drawings by Hooke and Huyghens more than 200 years ago.

Tke following papers were announced:—Two papers by Herr J. Plassmann—the first on "Algol," the second on "λ Tauri;" Mr. Espin on "Three new Red Stars;" Mr. Monck on "The Connection between Meteors and Comets;" Mr. J. Webb on "The Observatory of the late Mr. James Nasmyth." Two papers, "Observations of the Spectra of the Sun-spots," by the Rev. A. L Cortie, and "Some Notes on the Solar Prominences of 1890," by Mr. J. Evershed, Jun., were postponed to the next meeting.

ANNUAL MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

Wednesday, January 21.

Mr. W. H. Dines, B.A., *Vice-President*, in the Chair.

Dr. Tripe read the Report of the Council, which stated that the progress of the Society during the past year had been of a satisfactory character. Among the investigations carried on by the Society are the following:—The organization of a large number of meteorological stations, the observations from which are examined and reduced by the staff and printed in the 'Meteorological Record'; the regular inspection of these stations by the Assistant Secretary; the collection and discussion of Phenological Observations; and an enquiry into the Thunderstorms of 1888 and 1889. An Exhibition of Instruments is held annually in March. During the year, a complete Catalogue of the Library, extending to 222 pages, has been compiled and published. The Library has so much overgrown the present limited accommodation, that the Council have been obliged to consider the question of obtaining more commodious rooms, and have consequently inaugurated a "New Premises Fund," which is being well supported by the Fellows.

After the adoption of the Report, the Officers and Council for the ensuing year were elected.

At the Ordinary Meeting the following papers were read:—

1. "Note on a Peculiar Development of Cirrus Cloud observed in Southern Switzerland," by Mr. R. H. Scott.

2. "Some Remarks on Dew," by Col. W. F. Badgley. These are notes on observations which were made to discover whether all dew is deposited from the air, or if some also comes from the earth and plants, and also what quantity is formed during the year. The conclusions which the author deduces from his observations are:—(1) that the earth always exhales water-vapour by night, and probably a greater quantity by day; (2) that the quantity of water-vapour given off by the earth is always considerable, and that any variation in the quantity is mainly due to the season of the year; (3) that the greater part of the dew comes from the earth-vapour; and (4) that plants exhale water-vapour and do not exude moisture. The total quantity of dew collected on the author's grass plates in the year was 1·6147 inch.

Celestial Spectrum Analysis *.

The Potsdam observers have long been honourably known as astrophysicists who are also in the strictest sense astronomers;

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* Die Spectralanalyse der Gestirne. Von Dr. J. Scheiner. Mit einem Vorworte von Professor H. C. Vogel. Leipzig: Verlag von Wilhelm Engelmann, 1890.

and the literary embodiment of the activity of Potsdam on its most significant side, now offered to the world by Dr. Scheiner, shares the same satisfactory character. His work is thoroughly scientific. The reasonings contained in it are logical deductions from established principles; the facts are in general stated with the utmost practicable definiteness. Disagreements of fact with theory where they exist (and they are, in the present state of knowledge, inevitable), are honestly, if at times somewhat regretfully, allowed to be seen. They must indeed be unwelcome to the natural human impatience which survives even in the breast of an astronomer; but to serious students, open contradiction between phenomena and their theoretical explanation is infinitely preferable to a specious conformity, brought about by illegitimate means.

The methods, too, elaborately described by Dr. Scheiner from intimate personal knowledge, are marked, in every detail, by that laborious striving towards absolute accuracy through which the "new astronomy" has been rescued from the slough of loose conjecture and vague identification. The danger was imminent that its promising career might there end; since the data it afforded were liable to subtly compromising errors of novel kinds against which no established safeguards were at hand. But enthusiasm, kindled by the splendour of the half-opened prospect, combined with the unrelenting intolerance of error traditional in the older branch of the science to stimulate ingenuity; and by the separate, often minute efforts inspired by a common impulse towards a single end, the situation was saved.

The results of the victory, up to the present, so far as spectrum analysis is concerned, are collected in the work before us. It is divided into four parts, of which the first and second deal with the subject in its practical and theoretical aspects; the third states the facts ascertained by the means described in Part I. and seeks to interpret them by the principles enunciated in Part II.; while the fourth consists wholly of tabulated information. This is supplied with most praiseworthy lavishness. A table of lines in the visible solar spectrum contains 2614 measured by Vogel at Potsdam, besides 1406 further towards the red (here first published), determined by MM. Müller and Kempf with a large Rowland grating from the top of the Sentis, hence with slight disturbance from terrestrial atmospheric absorption. Unfortunately, however, the wave-lengths are given on a Potsdam scale, the introduction of which is equally unnecessary and undesirable. Angström's scale cannot, of course, be permanently retained; but Rowland's is its destined successor, and the multiplication of standards should be strenuously resisted. Captain Abney's wave-lengths for 590 lines in the infra-red solar spectrum form Table II.; Thalén's for 1204 components of the iron-spectrum follow; and in Table IV., stars with banded spectra, of both types, are jointly catalogued to the number of 994. A classified bibliography forms a useful Appendix.

Among the numerous data included in the body of the work, those relating to stellar radial movements have a special interest, as affording a conspectus of all extant results of the kind, from Dr. Huggins's epoch-making experiments, through the long series of Greenwich and Rugby visual measures, to the Potsdam photographic determinations of twenty-two stars, mostly of the solar type. These give an average velocity in the line of sight of 10½ English miles, Aldebaran, with a receding speed of 30½ miles per second, being the most rapid of the collection. Arcturus and α Cygni come out with the unexpectedly slow rates of approach of only 4·9 and 3·7 miles respectively; yet the ascription to the former at Tulse Hill and Greenwich of a velocity of about 50 miles seemed in great measure justified by the plates exposed with the first Potsdam apparatus in the early part of 1888 *. A practical certainty, none the less, that the star is really lessening its distance from us by less than five miles a second, has been afforded by Mr. Keeler's independent measurement of the D lines in its spectrum on four nights of last summer. The various determinations of Procyon also present curious anomalies. Dr. Huggins found it, in 1867, to be approaching at very nearly the rate established for it at Potsdam in 1890 of 6·7 miles a second; while spectrographic impressions of 1888 showed it, in conformity with Dr. Vogel's measures in 1871, as in extremely rapid retreat from the Earth. It is to be regretted that the presence of such grave discrepancies has not prompted Dr. Scheiner to undertake a discussion of them which would certainly have been instructive, and might have proved reassuring.

His postulate that the widening of the hydrogen lines in the spectra of Sirius and its congeners is due to their possession of very extensive atmospheres, needs proof. The spindle-shaped and downward pointed contour of the D_3 line, as viewed with a slit radial to the edge of the Sun †, emphasizes the risk of dealing at all confidently with the causes of spectral distensions. Possibly, however, the Algol stars will help to decide the point. For some of them, such as U Ophiuchi and S Antliæ, may turn out to be revolving in such close contiguity as actually to leave no room for far-spreading gaseous surroundings; while their possession by the eclipsing body should be betrayed by fluctuations in absorptive action, as yet undetected.

The opinion, favoured by our author, that bright lines in stellar spectra are a mere effect of preponderance in extent of an atmospheric over a photospheric radiating surface, is obviously irreconcilable with the fact, well-ascertained in two noted instances, of their variability. Nor can the system of classification, by which gaseous stars are attached as three distinct subordinate varieties to the three chief spectral orders, be deemed commendable unless it be regarded as provisional. No definitive arrangement of them should indeed as yet be attempted.

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* Sitzungsberichte, Berlin, March 15, 1888.

† Vogel, Bothkamp Beob. Heft ii. p. 36; Scheiner, Die Spectralanalyse, p. 202.

On a few of the points touched upon in the valuable treatise before us, rectifications suggest themselves as desirable. Thus, at p. 242, the relations of aspect and brightness in the Pons-Brooks Comet are reversed. Normally faint and diffuse, it assumed stellar sharpness during its outbursts of light on September 22, 1883, and January 1, 1884*. Moreover, Dr. Huggins's recognition of the true quality of cometary light as exemplified in Winnecke's Comet (1868) ought to have been, but is not, mentioned at p. 227; while the same observer is credited (p. 214) with holding the view, explicitly disavowed by him in 1867†, that the red colour of Mars is of atmospheric origin. Dr. Scheiner makes no mention of the blue line at about $469^{\mu\mu}$, recorded by Drs. Huggins and Copeland in three planetary nebulæ (Gen. Cat. 4964, and two in Cygnus). Yet it is of peculiar significance from its probable identity with the blue band of the Wolf-Rayet stars, as well as with a line temporarily radiated by Nova Cygni. Finally, the survival, in a work of such importance, of the nitrogen-association of the chief nebular line, is particularly unfortunate just now, when the question of its origin is under close investigation. Dr. Huggins in particular, cited as the leading authority for the identification, observed indeed in 1864 and subsequently, a "sensible coincidence" between the green ray common to all gaseous nebulæ and the brightest air-line, but never unreservedly adopted a chemical interpretation of an agreement which his own later researches have shown to be merely approximate; and for many years past has used the nitrogen-line equally with the adjacent lead-line, simply as a convenient standard of reference.

The occurrence, however, of a few minor blemishes does not seriously impair the usefulness of a production which ought to serve—either in the original or translated—as a text-book to every worker in sidereal physics. The want of such a one, long felt, is now ably supplied. A word of praise may be added for the simple and straightforward style in which it is written. The vices of cumbrousness and complication, too prevalent in German scientific literature, are happily avoided; there are few, or no "wounded snake" sentences to be found in it, nor any of the *mots d'enflure* which Pascal detested. Some really exquisite reproductions of photographed stellar spectra are exhibited in two plates at the end of the volume.

A. M. CLERKE.

Comet-seeking at Bristol.

THE following summary for the last eighteen months may not be altogether without interest, though the results are meagre, the observations having been much interrupted by my ill-health. The instrument used was a 10-inch silver-on-glass reflector, powers 32, 40, and 60, with fields of 75′, 65′, and 50′ respectively.

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* Astr. Nach. Nos. 2553, 2568.

† Month. Notices, vol. xxvii. p. 180.

Number of nights of observation	78
Hours of observation	155½
Telescopic meteors observed	141
New comets discovered	1
New nebulæ discovered	10

During the progress of the work I also recorded 16 brilliant naked-eye meteors. It may be worth while referring more fully to the individual results.

Telescopic Meteors.—I find that during the last half of the year about one meteor per hour is seen in the comet-seeker, but during the first half the number is more scanty, the average being one in two hours. This accords with the relative frequency of naked-eye meteors, and proves that the two classes of these objects are identical as regards the variations in their distribution over the year. My former estimate that telescopic meteors are more frequent than naked-eye meteors in the proportion of about 22 to 1, is confirmed by these recent observations. The opinion that the former are at great heights in the terrestrial atmosphere, appears very probable from their slow motions and short courses. Many of them exhibit curved paths, though this feature is not often marked in regard to naked-eye meteors.

New Comet.—Comet *c*, 1890, was discovered on July 23. It was a small faint nebulosity, in about 228° +78°, and moving southwards nearly 1° per day. It was seen at Nice on July 24 and 25, at Mount Hamilton on July 25, and at many other observatories. The perihelion was attained on Sept. 24, at which time the comet was situated near δ Serpentis, and nearly twice as bright as on the date of discovery. Prof. E. E. Barnard, with the 12-inch refractor at the Lick Observatory, made a valuable and numerous series of observations extending over the period from July 25 to October 6, and his results were published in No. 228 of Gould's 'Astronomical Journal.' Prof. Barnard notes that "the comet remained a faint object throughout the observations. In July and August it had a small stellar nucleus of the 13th magnitude, and a faint diffused tail." Prof. Lewis Swift, with the 16-inch refractor at Rochester, N.Y., observed the comet on three occasions, and says that "it had a short, broad tail." Prof. W. R. Brooks, with the 10⅛-inch refractor at Phelps, N.Y., saw the comet on August 12 and 16, and found it "faintish but not difficult." A parabolic orbit satisfies the observations. After its perihelion the comet passed rapidly into the southern hemisphere.

New Nebulæ.—Ten new nebulæ were discovered, all of them in, or on the borders of, Camelopardus, and not far from the north pole. A list of these objects was given in the 'Monthly Notices' of the R.A.S., December 1890, p. 96. Eight of the nebulæ enumerated are tolerably conspicuous with a power of 60, and might be seen with a much smaller aperture than 10 inches. I met with no new nebulæ in places far distant from the pole, and there is no doubt that the circumpolar region is a promising one for further discoveries. It appears to have been much neglected owing to the difficulty of using large equatoreals upon it. Other portions of the firmament have been pretty thoroughly swept, and there are comparatively few nebulæ (plain enough to be picked up with the instruments and powers usually employed in comet-seeking) that still await detection. The space surrounding the pole certainly needs a closer examination than it has hitherto received, for although the more prominent objects are already catalogued, many others must remain which are fairly distinct in moderate apertures.

It is often the case that several nebulæ are clustered near together. Whenever a new one is discovered the surrounding region should therefore be carefully surveyed in search of others. The space immediately outlying known objects may also be regarded as prolific ground for new discoveries. Several of the nebulæ recently discovered at Bristol are close to known objects; thus, on Nov. 7, 1890, I encountered Tempel's pretty large, pretty bright nebula at R.A. $7^{h}\ 5^{m}\ 24^{s}$, N.P.D. 9° 34′·6 (N.G.C. 2336), and on examining the region near, soon alighted upon another, 22′ SSf., which had been previously overlooked. I find that after several hours of comet-seeking, faint nebulæ, scarcely obvious at the commencement of work, become very easy objects, the eye being in a much better condition to receive feeble impressions.

Naked-eye Meteors.—Several fireballs and bright flashing meteors were seen casually, and their paths registered with as much accuracy as circumstances allowed. Some of these meteors were recorded at other places, but the published descriptions are not sufficiently definite to enable their real paths to be ascertained. If telescopic observers would always pause to record the large meteors which occasionally obtrude on their attention, we should soon possess a valuable mass of materials. Nearly all the fireballs escape satisfactory investigation owing to the lack of details in the accounts of observers. The apparent course amongst the strs and the duration of flight should always be given as correctlyas possible.

W. F. Denning.

Bristol, 1891, Jan. 9.

The Chromosphere in 1889 *and* 1890.

The following tables taken in conjunction with the corresponding table for 1888, communicated to the 'Observatory' by the late Father Perry (March 1889), show that the period of least disturbance of the chromosphere covered the 12 months of the year 1889, at least so far as the small number of observations allow.

A marked change appears in January 1890, with a decided tendency to a rapid increase of activity, especially in extent, on the limb. But unfortunately the last two months of the year were lost through the excessive cloudiness of the season and the ill-health of two assistants.

The mean height of the chromosphere remains, as has been already noted in past years, very constant; but it is not without local variations. The range or difference between the highest and lowest measurements of the chromospheric depth was between 2″ and 3″ of arc on eight days, and between 3″ and 4″ on two days of 1889. And in 1890 we find the range between 2″ and 3″ on nine days; between 3″ and 4″ on four days; between 4″ and 5″ on two days; between 5″ and 6″ on three days; and between 6″ and 7″ on three days.

Occasionally an extended depression of the chromosphere has been observed near the solar equator. In 1889 this depression was noted, in the same locality, on the 2nd, 5th, 6th, and 7th of May. And assuming that the depression remained continuous during the interval of five days, its area was very great, having a mean extent in latitude of 40° and a total length in longitude not less than 66°. Another extensive depression is recorded for May 23, 25, and again for June 6, 8. In 1890 we find two coexisting depressions at the beginning of April between the 1st and 4th day—one on the preceding, the other on the following limb.

Stonyhurst College Observatory. WALTER SIDGREAVES.

The Chromosphere in 1889.

	No. of days of observation.	Mean height of chromosphere excl. prominences.	Mean height of prominences.	Mean extent of prominence in arc.	Highest prominence.	Number of days on which C was displaced.
		″	″	° ′	″	
January	3	8·24	14·10	5 22	39·30	0
February ..	7	8·12	16·71	3 36	42·72	0
March......	8	8·12	10·29	1 20	59·81	0
April	4	8·12	15·17	2 41	29·90	0
May	10	7·90	9·52	2 14	39·30	0
June	15	7·90	14·70	1 40	61·52	1
July	7	7·86	14·23	2 30	44·00	0
August	4	7·56	8·63	0 24	36·32	0
September ..	11	7·86	9·65	2 2	46·99	1
October	4	7·90	11·10	2 13	34·61	0
November ..	1	8·12	28·19	1 39	34·18	0
December ..	3	7·82	5·68	0 40	17·09	0
Mean for the Year	6·4	7·96	13·16	2 12	40·48	0·2

The Chromosphere in 1890.

	No. of days of observation.	Mean height of chromosphere excl. prominences.	Mean height of prominences.	Mean extent of prominence in arc.	Highest prominence.	Number of days on which C was displaced.
January	4	8″·29	24″·44	6° 8′	46″·99	0
February ..	6	8·24	34·04	6 28	67·07	0
March......	5	7·69	24·86	5 26	49·98	0
April	7	7·69	24·39	5 4	42·72	0
May	10	8·20	29·82	8 14	70·49	0
June	5	8·12	26·91	9 0	53·40	0
July	2	7·99	29·26	14 59	51·26	0
August	3	8·20	19·65	13 24	38·45	0
September ..	4	8·03	26·70	12 16	42·72	0
October	4	8·07	18·16	5 13	38·45	0
November ..	0					
December ..	0					
Mean for 10 months	5·0	8·05	25·82	8 37	50·15	0

The Great Excentricities of the Stellar Orbits.

THE marked contrast between the small excentricities of the orbits of the planetary system and the high excentricities of the orbits of binary stars is well known. The arithmetic mean excentricity of 50 of the best stellar orbits hitherto computed is nearly five-tenths, while the mean excentricity for the planetary orbits is less than one-tenth of this fraction. The highest stellar excentricity known is that of the orbit of γ Virginis, which is nine-tenths, or almost as high as that of the orbit of Halley's comet. Beyond question these great excentricities have been developed by the continual action of some physical cause. A rigorous dynamical investigation, too long and difficult to detail here, has led the writer to the conclusion that the great excentricities observed have arisen from the continual action of tidal friction. It is not difficult to show that where the time of revolution exceeds that of rotation the excentricity of the orbit will generally increase. And the great importance of the action of tidal friction in the binary systems will be evident when we remember that these are systems composed of two stars very

Table of Binary Orbits, showing Excentricities.

Name of Star.	Excentricity.	Calculator.
ξ Scorpii—Σ 1998..	0·0768	Doberck.
221 (B) Ophiuchi—Σ2173	0·1349	Dunér.
OΣ 387	0·139	Glasenapp.
μ^1 Herculis—A.C. 17—Σ 2220	0·14853	Celoria.
δ Equulei—Σ 2777	0·2011	Wrublewsky.
12 Lyncis—Σ 948	0·229	Gore.
η Cor. Bor.—Σ 1937	0·2667	Doberck.
μ^2 Herculis—Σ 2220	0·3023	Doberck.
Σ 1819	0·3052	Casey.
Σ 3121	0·30863	Celoria.
δ Cygni—Σ 2579	0·327	Gore.
Castor—Σ 1110	0·3292	Doberck.
τ Cygni	0·3475	Gore.
85 Pegasi—β 733	0·35	Schaeberle.
β Delphini—Σ 2704	0·3567	Dubiago.
OΣ 234	0·3629	Gore.
ζ Cancri—Σ 1196	0·365	Seeliger.
6 Eridani—H_2 612	0·378	Doberck.
Σ 2107	0·3867	Berberich.
γ Cor. Bor.—Σ 1967	0·387	Doberck.
OΣ 20	0·416	Glasenapp.
ξ Ursæ Maj.—Σ 1523	0·416	Pritchard.
OΣ 489—π Cephei	0·434	Glasenapp.
Σ 3062	0·4472	Doberck.
OΣ 149	0·460	Glasenapp.
4 Aquarii—Σ 2729	0·4613	Doberck.
ζ Herculis—Σ 2084	0·4627	Doberck.
70 Ophiuchi—Σ 2272	0·4728	Tisserand.

Name of Star.	Excentricity.	Calculato
42 Comæ Ber.—Σ 1728	0·480	O. Struve.
λ Ophiuchi—Σ 2055	0·4930	Doberck.
μ Draconis—Σ 2130	0·4930	Berberich.
OΣ 4	0·506	Glasenapp.
Σ 298	0·51	Dolgoruko
α Centauri—H_2 6047	0·5332	Doberck.
Σ 228	0·5311	Gore.
ω Leonis—Σ 1356	0·5360	Doberck.
η Cassiopeiæ—Σ 60	0·5763	Doberck.
OΣ 298	0·5836	Celoria.
OΣ 235	0·5870	Doberck.
μ^2 Boötis—Σ 1938	0·5974	Doberck.
OΣ 413—λ Cygni	0·602	Glasenapp.
τ Ophiuchi—Σ 2262	0·6055	Doberck.
Sirius	0·6148	Auwers.
Σ 1757	0·6233	Casey.
ζ Aquarii—Σ 2909	0·6518	Doberck.
36 Andromedæ—Σ 73	0·6537	Doberck.
Σ 2107	0·6688	Casey.
γ Cor. Australis	0·6989	Schiaparell
ξ Boötis—Σ 1888	0·7081	Doberck.
44 Boötis—Σ 1909	0·71	Doberck.
25 Can. Venat.—Σ 1768	0·7221	Doberck.
γ Leonis—Σ 1424	0·7327	Doberck.
De. 2091^2	0·7352	Gore.
σ Cor. Bor.—Σ 2032	0·7502	Doberck.
φ Ursæ Maj.—OΣ 208	0·788	Casey.
99 Herculis—A.C. 15	0·7928	Gore.
γ Virginis	0·896	Thiele.

much like our Sun, and that the component stars of a system are always relatively of the same order of mass. The planets, on the contrary, are of an entirely different order of mass from that of our Sun; and, as Prof. G. H. Darwin has shown, tidal friction has not been of the first importance in the evolution of the planetary system. But the independent investigation above mentioned, wherein the writer has followed methods somewhat analogous to, but also considerably different from, those of Prof. Darwin, seems to show beyond doubt that the secular action of tidal friction has been of the highest importance in the history of binary systems. From this research it appears most highly probable that a binary star was formerly a single nebula, that it split up into a double nebula, and that finally the double nebula separated into two comparable masses, whence by the continual operation of tidal friction the components were wound off to a great mean distance, where they continue to revolve in an orbit greatly enlarged and made very excentric by the action of tidal friction in the bodies of the stars. It is remarkable that if the separation has taken place in this way—which would imply that both stars rotate in the same sense in which they revolve—the tidal reaction of either star tends to increase both the mean distance and the excentricity of the orbit. The rigorous mathematical method by which these conclusions have been reached will be published in due time; the object of this note will have been achieved, however, if it serves to indicate the great importance of the action of tidal friction in a system of two large fluid bodies comparable in mass, and to point out to astronomers the physical meaning of the hitherto perplexing high excentricities of the stellar orbits. The binary systems are therefore radically different from the solar system in two respects :—

1. *The high excentricities of their orbits.*—A table of 50 of the best orbits presents every phase of elongation from almost planetary circularity, on the one hand, to an excentricity almost equal to that of Halley's comet on the other.

2. *The relatively large mass-ratio of the components of a system.*—This latter characteristic ensures great predominance to the tidal forces, more especially in past ages when the stars now widely separated were closer together and more expanded in volume. It is also to be observed that where the masses are comparable, either star exercises an important tidal reaction on the other, and this renders the investigation of their history very difficult. In treating of the binary stars the tidal reaction on the system arising from the rotation of the smaller body cannot be neglected.

Finally, the writer begs to add that the research into the history of the binaries has led him to interesting conclusions respecting the process by which a single nebula splits up into two comparable masses, and the system eventually develops into a binary star. A discussion of this process will, however, be reserved for another article, and at present we may merely remark that the general process of cosmic evolution seems to be that exemplified in the double nebulæ and the binary stars. The development of the solar system seems to have been an exception and not the rule. From these considerations the writer would venture the opinion that investigators of cosmogony who have looked upon the solar system as typical of the general process of cosmic development, and proceeded therefore to investigate stellar evolution in general, have pursued an erroneous path. Any safe basis for cosmical speculation must be reached through a study of the binary systems and not through an investigation of our own remarkable planetary system. All the stellar systems hitherto discovered are radically different from our own; and we may even affirm that if systems such as the planetary system exist in the heavens, they have at least not yet been discovered. So that whilst we do not say that there are no other planetary systems—composed of one great central body surrounded by insignificant planets—we have at least no knowledge of such, and can merely affirm that all the known stellar systems are of an entirely different character. If the conclusions here indicated be correct, and there seems to be no doubt in the minds of several astronomers to whom the writer has presented the original work, we perceive that no small amount of new light will be thrown upon many points of stellar cosmogony which hitherto have been very obscure. T. J. J. SEE.

University, Berlin, Prussia, 1891, Jan. 20.

Selenographical Notes.

LUNAR PHOTOGRAPHY.—Through the courtesy of Dr. Holden, I have received a silver print of an enlargement of a negative taken by Mr. Burnham with the great Lick telescope, 1890, August 31, $14^h\ 27^m$ P.S.T., which may be regarded as the most successful and valuable work of its kind which the camera has yet accomplished in the field of selenography. It represents that portion of the Moon's surface extending from Cleomedes on the north to Vendelinus A on the south; on the west the evening terminator grazes the west wall of Langrenus and the western side of the Mare Crisium, while on the east the picture includes Isidorus and Römer. It is the result of an enlargement of three times of the original negative, and shows the Mare rather more than two inches in extreme length. In spite of the inevitable exaggeration of half-tones, the profusion of objects on the complicated border and on the eastern side of the floor, with Pierce, Pierce A, and the light streaks, is shown with marvellous fidelity. Taruntius, with its inner concentric ring, Macrobius with the detail on its floor and wall, the triple central mountain of Cleomedes and the associated group of craters (A) on the northern quarter of its interior, and other depressions within its border, are also brought out with extraordinary sharpness. The magnificent ring Langrenus on the evening terminator is likewise reproduced with admirable distinctness. Messier and Messier A, with the comet-like streaks on the east of the pair and the surrounding craterlets and faint markings, show what may be done with respect to objects of a smaller and more delicate type. This print leads one to hope that the achievements of the camera in the future may surpass the most sanguine expectations, though we may have long to wait ere it succeeds in portraying delicate clefts and other minute detail revealed to the eye.

SANTBECH.—The bright plain lying between Fracastorius and the southern end of the Mare Fœcunditatis is remarkably deficient in features of interest, Santbech being the only formation of any size or prominence which diversifies an otherwise barren region—a circumstance which perhaps renders this ring-plain more noticeable and imposing than would be the case if there were other conspicuous objects associated with it. It is a noteworthy formation, nevertheless, some 50 miles in diameter, presenting, both at sunrise and sunset, a fine picture with a moderately high power, and exhibiting many interesting features to the selenographer, though in many respects it is seen to best advantage when the Sun is setting. Under these latter conditions, there are few formations of its class which afford a more instructive example of a wide *glacis* or counterscarp, the slope of the eastern wall extending at least six or seven miles before it finally makes junction with the surrounding plain. The irregularity of the line marking the union of slope and plain is also worthy of note, especially on the north-east, where it displays a scalloped outline, due doubtless to the shape of the ground. On the south and south-east the slope runs out to a distance of at least ten miles from the summit of the wall, and exhibits indications of landslips in two or three places. Neison gives the height of this wall as 15,113 feet above the interior—a measurement which, though some 2000 feet in excess of Schmidt's value, affords a slight idea of the vast scale on which it is raised. The west wall approaches 10,000 feet in altitude, so that this formation may be classed among those which have the loftiest borders. Near the summit of the east wall stands a bright large crater of no great depth, which is not shown by Neison, and drawn much too far to the north by Mädler, if the prominent object he shows on the north-east side of the ring is intended for it. At the foot of the northern outer slope of the border is a large horseshoe-shaped cavity, like a deep crater, with its north wall destroyed; and adjoining this on the north is a complete crater, somewhat smaller, standing at the southern end of a wide valley leading towards Colombo. It is very strange that neither Mädler nor Neison show a trace of the large bright depression which figures on or very near the summit of the giant peak on the west wall, as, at sunset especially, when this wall is on the terminator, it is so conspicuous, through causing a notable projection in the black shadow, that one would think it could not possibly be overlooked. The abnormal darkness of the tone of the monotonous plain on the east of Santbech under a low evening Sun and the brightness of the same area under a more vertical illumination is a phenomenon awaiting explanation. There is no doubt as to the fact. Though the plain is undoubtedly devoid of any lofty hills or even ridges of magnitude, there are many low mounds and minor irregularities, but nothing apparently which can account for the district resembling a Mare in colour for a few hours under an oblique light and a bright highland at other times. Santbech *c* is a very small ring-plain on the south-east, worth examining, with a minute crater on its western flank. North of this are two short crater-rows, each consisting of three craters Adjoining the west side of that nearer Santbech is a curious large, dark, ill-defined spot of a doubtful nature; and between this and Santbech two large circular depressions, resembling "veiled rings," which give one the idea that they have been partially obscured by subsequent deposits of some kind or other. THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1891, Jan. 16.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

U and T Tauri.

Gentlemen,—

Reference is made in Miss Clerke's charmingly written volume 'The System of the Stars' (p. 183) to a star which his observations in 1862 led the late Mr. Baxendell to regard as variable with a range of rather more than a magnitude, and which was lettered by him U Tauri. The star in question is D.M. $+19^{\circ}\cdot705$, 9·4 mag., its place for 1855·0 being R.A. $4^{h}\ 13^{m}\ 20^{s}\cdot9$, Decl. $+19^{\circ}\ 28'\cdot1$. The star was first observed by me on Nov. 30, 1863, when I thought it to be of about 9 magnitude. After observing it occasionally in the several following years as a single star, I found it on Dec. 4, 1867, to be a moderately close double, the components equal or nearly so, and measures with the filar micrometer on Dec. 5, 1867, and Feb. 6, 1868, gave $P=22^{\circ}\cdot10$ or $202^{\circ}\cdot10$ and $D=3''\cdot10$, the measured distances being larger than my estimates had led me to expect. From a note on p. 3 of Schönfeld's 'Zweiter Catalog von veränderlichen Sternen,' the star appears to have been seen double by D'Arrest in the same year, 1867, and observations by Prof. Schönfeld, and Dr. Tietjen with the Berlin refractor on March 21, 1871, gave an estimated distance for the components of $4\frac{1}{2}''$, with position-angle 190°, the southern star being slightly the fainter of the two.

I have had the star under occasional observation up to the present year, and have generally noted the magnitude of the components, viewed with a low power as one star, about 9·4, occasionally a few tenths brighter. There is no decided colour, with the exception of an observation on Oct. 19, 1867, when I have the note "slightly ruddy." The components have generally appeared to be very nearly equal in magnitude, though the *s.p.* star has on many occasions been rated as slightly the larger. On two occasions only, Oct. 15 and Dec. 2, 1879, has the *n.f.* component appeared slightly the brighter of the two. On Dec. 12, 1888, the *s.p.* star was noted "slightly ruddy." Assuming 9·4 as the combined magnitude of the pair, the magnitude of each component would be about 10·2. So far as my own observations are concerned there would appear to be no decided evidence of change in the magnitude of the star, and I believe that the late Mr. Baxendell was inclined to doubt whether there may not have been some error in his earlier observations. I hope, however, to keep it under occasional observation.

About 17′ to the south of U Tauri, 10^{s} *f.* lies Hind's remarkable variable T Tauri, D.M. $+19^{\circ}\cdot706$, closely associated with a variable nebula, some interesting observations of which made by Mr. Burnham with the 36-inch refractor of the Lick Observatory are printed in the December number of the 'Monthly Notices.' T Tauri has a magnitude range of from 9·4 to 13·5 or

14, but no regular period has as yet been detected. I have had it under observation, with greater or less regularity, for the past twenty-seven years (with a gap of four years, 1873–1876), and can only describe its changes as "irregular," unless the period is one of some years' duration, with minor fluctuations on the rising and falling curve. During the years 1877–1882 the star appears to have fluctuated about a maximum, with range 9·4 to 10 mag., and its tint was then noted by me "slightly ruddy," "ruddy," "ruddy lilac," "lilac," and once "bluish white." It has since gradually declined in magnitude and is now near minimum. Its magnitude was noted 13·4 on Jan. 1 of the present year. I have never seen any certain trace of Hind's nebula, but on two occasions in 1877 suspected a minute glimpse star near the place. The collection and discussion of all available observations of T Tauri might perhaps lead to some more definite conclusion in regard to its period.

Yours faithfully,
George Knott.

Knowles Lodge, Cuckfield.
1891, Jan. 16.

Parallax and Velocity of Double Stars.

Gentlemen,—

My attention has been lately directed to a paper in the 'Monthly Notices' of the R.A.S., May 1874, in which Prof. Niven shows how the parallax and relative velocity in the line of sight of a double star are connected with the elements of its orbit. As I have already published two papers on the same subject, one in the 'Proceedings of the Royal Irish Academy,' May 1886, and the other in the 'Monthly Notices,' March 1890, in which I have given formulæ almost identical with those of Prof. Niven, I should like to be the first to draw attention to the coincidence.

At the time of writing I thought that my geometrical examination of the question had at least the advantage of novelty; and though I may perhaps venture to hope that my method of approaching the question being slightly different from that of Prof. Niven will be considered not wholly without value, and that my figures, as being based on more recent and more trustworthy data may be more reliable than his, I doubt if I should have ventured to publish either one or the other had I been aware that I was on an already beaten track.

Yours faithfully,
Arthur A. Rambaut.

Dunsink,
1891, Jan. 19.

Uncatalogued Comets.

Gentlemen,—

A perusal of Calvisius indicates two or three comets that seem to have escaped notice in our Catalogues.

A.D. 535. "Hac hyeme, cum sol in Capricorno esset, ingens cometa apparuit in Sagittario."

A.D. 599. "Cometa hoc anno apparuit." No comet mentioned even in Williams's Chinese list from A.D. 594 to A.D. 607.

A.D. 1505. "Cometæ bini annotantur hoc anno; primus apparuit 12 Aprilis, alter mense Augusto." Chambers mentions one in Aries this year, but here we have mention made of two.

To these might be added "the star of marvellous size with one single ray," described by Matthew, of Westminster, as seen in our land in A.D. 497. [See 'Journal of British Astr. Assoc.,' Nov. 1890, p. 80.]

Faithfully yours,

S. J. JOHNSON.

Melplash Vicarage, Dorset,
1891, Jan. 8.

OBSERVATORIES.

STONYHURST.—An interesting paper by Father Cortie on Sun-spot Observations made at Stonyhurst in the years 1888–89 appears in the last number of the 'Monthly Notices.' The last important contribution to Astronomy which came from this Observatory was the late Father Perry's memoir on photographs and drawings of the Sun (Mem. R. A. S. vol. xlix.), in which he showed so well how there is room for new workers in an apparently well-occupied field. We may remark in passing that a meeting of the Father Perry Memorial Committee will be held on Feb. 13, before the Annual General Meeting of the R. A. S.

Father Cortie's work has been thorough and painstaking. He sums up his conclusions under 20 heads, and it seems scarcely necessary to reproduce them here.

RIO DE JANEIRO.—Volume IV. of the 'Annales' has been published in two parts, dated 1889, though we have only lately received them. The second contains meteorological observations only. The first, of 126 pages quarto, contains three sections, viz.:—(1) an essay on the distribution of asteroids, written in 1879, in which it is argued that these bodies may be the fragments of a planet split up, not by one, but by several catastrophes at different points of its orbit—these catastrophes being cometary encounters, of which there is evidence in the distribution of the orbits; (2) an account of the observations of the transit of Mercury in 1878, previously published; (3) micrometric measures of double stars.

Observations of southern doubles are not very numerous, so that it was with some pleasure we found that M. Cruls had devoted a considerable time in the years 1879–80 to their measurement with his 0″·25 refractor. Alas! not more than 5 per cent. of the measures are of permanent value. It is, we believe, generally recognized that measures of new doubles over 30″ apart are of very small value. W. Struve limited the distance to 32″, and O. Struve still further reduced it to 16″. Other observers have certain restrictions, but perhaps Burnham's scale is the safest. A comparison of the measures before us with β's limits will give some idea of our disappointment. Column 1 gives the magnitude of the principal star; col. 2 Burnham's limit of distance; col. 3 the mean distance of stars measured by M. Cruls; col. 4 his max. and min. distances; col. 5 the number within limit; col. 6 those outside limit.

Mag.	Limit of Distance β.	Mean Distance.	Max. & Min. Distances.	Number of Stars	
		Cruls.		within limit.	without limit.
	″	″	″ ″		
10	1	44	118 – 3	0	17
9	2	29	140 – 2	1	20
8	5	48	190 – 1	6	32
7	9	60	335 – 1	10	24
6	15	51	259 – 1	10	12
5	25	46	216 – 1	16	15
4	35	..	......	..	..
3	45	78	250 – 7	3	2
2	55	77	245 – 6	3	7
1	70	84	151 – 7	1	2

There are a few measures of α^1 and α^2 Centauri, π Lupi, and H_2 5014. We greatly regret that M. Cruls did not spend the time on more interesting stars. T. L.

Rousdon.—Mr. Peek reports that 1890 was on the whole a favourable year, always excepting December. Transit observations were taken on all available occasions, and the time service was fully maintained. The number of observations of the selected 24 long-period variables shows a steady increase, being 451 for 1890, as against 123, 284, 332, and 444 for previous years from 1886.

PUBLICATIONS.

Astronomical Lessons*.—A little book of 130 pages, writter in a simple style, and well adapted for the purpose indicated o the titlepage. There are 19 separate chapters, which thus occup only a few pages each. That on nebulæ, for instance, barely fil two. The author has thus endeavoured to avoid confusing t tender minds of his readers—an admirable object to keep in view

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* Astronomical Lessons, or Chapters on the Elementary Principles a Facts of Astronomy, for the use of Students and Young People. By J. E. G F.R.A.S., &c. Sutton, Drowley, & Co. 1890. Price 3*s*. 6*d*.

We could wish, however, that Mr. Gore in his efforts to be simple had not been quite so slipshod. Here are a few of his expressions:—"The bounding-line of the horizon" (p. 16); "that any such appearance could ever be seen" (p. 17). "For a sphere, when the source of light is a globe larger than the sphere (as in the case of the Sun and Earth) the shape of the shadow is always a cone" (p. 76). [What is the difference between a globe and sphere? and why not state the proposition more completely?] "The shape of its (Mars's) orbit is, however, more elliptical than that of the Earth. For this reason it is sometimes much nearer to the Earth than at others, and consequently varies considerably in brilliancy at different oppositions." [A young person might easily be misled into thinking that with circular orbits Mars would be always at the same distance from the Earth.]

We believe it is somewhat a new departure for Mr. Gore to deal with mathematical rather than purely descriptive astronomy; and he may think the above sentences clear enough for the purpose. But they jar on the nerves of those accustomed to look for accurate expression in such work.

MERIDIAN PHOTOMETRY.—Vol. xxiv. of the Harvard Annals contains the results of observations with the meridian photometer in the years 1882–1888. We are referred to Vol. xxiii. (which, however, is not yet to hand) for a discussion of these results, the present volume being simply the Catalogue, with a few words of introduction. It will be remembered that Volume xiv. of these Annals was called the 'Harvard Photometry' (H.P.), being a catalogue of the magnitudes and approximate places of 4260 stars of the sixth magnitude and brighter. After the completion of this work a new meridian photometer was constructed, with object-glasses 4 inches in aperture instead of 2. The field was 40′ in diameter, and the magnifying-power employed was 28. By its means stars of the ninth magnitude could be readily observed, and those of the tenth could be measured under favourable circumstances. The principal work undertaken with this new instrument was the determination of the magnitudes of a sufficient number of *stars* contained in the Durchmusterung, and distributed with approximate uniformity, to serve as standards of reference for future estimates or measures of magnitude. The result is the present catalogue of 20,982 stars, which may be designated H.P. (2) or perhaps M.P. (Meridian Photometry); though the latter combination of letters has already a baser use on this side the Atlantic. The work is obviously excellent both in quantity and quality; and we learn with some surprise and admiration that two-thirds of the observing was done by Prof. Pickering himself. The reductions and the other third of the observing are due to Mr. Wendell. We look forward with great interest to the appearance of the discussion in Vol. xxiii.

NOTES.

Comets Zona and Spitaler.—We have perhaps not called sufficient attention to the strange coincidence by which one comet was discovered when looking for another within a few minutes of the place in which the already known comet really existed. Our contemporary the 'Sidereal Messenger' reprints from the 'San Francisco Examiner' a note by Mr. Barnard on this unprecedented occurrence. It will be remembered that Dr. Spitaler on 1890 Nov. 16, when looking for the comet discovered by Prof. Zona on the previous evening, found what he at first supposed to be Zona's Comet; and afterwards swept into the same field the real object for which he was looking, which was subsequently found to have no physical connection with the one he first observed. "There is no record of any two comets, not physically related, ever having been seen within a degree of each other....These two comets are being (Dec. 10) regularly observed at the Lick Observatory. Neither is bright. Spitaler's is exceedingly small and faint—the merest speck of haze moving slowly among the stars." In No. 232 of the Astr. Journal the Rev. G. M. Searle computes elliptic elements for Comet Spitaler, with a period of about 13 years. "The elements are still of course quite rough; but it would seem probable that the period is such as to place this comet among those subject to special perturbation by Saturn, and possibly brought into one system by that planet."

A Monument to Fabricius.—In the churchyard of the village Osteel, in the north of Holland, is a weather-worn gravestone bearing the legend "Anno 1617 d. 7 May is de würdige un wolgeleerde Heer David Fabricius, Pastor un Astronomus tho Osteel, van eenen geheten Frerik Hoyer jammerlyken vermoordet in't Jaer 53 sines Olders." The Naturforschenden Gesellschaft propose to erect a more lasting monument to this great man, and have headed the subscription list with 100 marks. Further contributions (up to 1891, May 1) will be gratefully received by Herr Kaufmann Peter de Jonge, Gr. Osterstrasse, Nr. 46, Emden, Ostfriesland.

It is curious that Newcomb, speaking of Fabricius (Pop. Astr. 2nd ed. p. 250) and his early observations of the solar spots, says that he was "otherwise unknown to astronomy," forgetting apparently his discovery of the variability of Mira Ceti. This, according to Wolf, was made known in a letter to Tycho Brahe. Kepler gives the date of Fabricius's observation as 1596, Aug. 13 (N.S.), when he saw it as 3rd mag., and in October of the same year he noticed that it was no longer visible.

Double Stars.—No. 2978 of the Astr. Nach. contains a very useful series of measures, by Prof. Hough, of 107 double stars and also of 94 new doubles. Three, at least, of the latter (Hough 194, 210, 225) ought to receive special attention from possessors of large telescopes. The measures were made partly in the city of Chicago at the old site of the Dearborn Observatory, and the more recent (since August 1889) at the new site in Evanston, Illinois. The refractor is of 18½-inch aperture.

On Thursday, Jan. 29, Prof. George Darwin delivered the Bakerian Lecture at the Royal Society on tide prediction. He remarked that 50 years ago, Whewell had delivered a Bakerian Lecture on the same subject. Since then, our knowledge of the methods of analyzing and computing tides had greatly advanced, though we had still much to learn about the actual tides themselves. Whewell, for instance, did not realize that the tides in the North Atlantic, such as we were accustomed to in Europe, were quite abnormal, and that in all the rest of the world the diurnal tide was not insensible. For this reason the simple tide-tables such as are used in Europe could not be formed; or, if formed, are practically useless. A much more elaborate calculation must be made and expense was unavoidable; it remained only to consider how the expense might be minimized. The tide-predicting machine, depending on the method of Harmonic Analysis, devised by the President (Sir W. Thomson) in 1872, would draw a curve representing all the tides at any port for every day in the year in about four hours; but then the daily measurements of this curve must be made and verified; and the cost of the machine was several thousand pounds; so that the annual charge for supplying a port with tables for every day in the year was still necessarily somewhat high. Again, only one of these machines was in existence, and other governments might be deterred by national pride from sending to London for their predictions. The lecturer had accordingly devised a method of supplying to any port which would incur the expense of its computation a general table, applicable for all time, by means of which any one with an elementary knowledge of the 'Nautical Almanac' might, in a few minutes, compute two or three tides for the days on which they were required. The general table might be constructed so as to furnish results of varying degrees of accuracy, with corresponding differences in the expense incurred. The less elaborate form of table would be suitable in cases where existing observations of tides were not numerous or very accurate; for it was of course from existing tidal observations at the port that the general table was to be constructed.

After some congratulatory remarks from Commander Creake on behalf of the Hydrographer, and from the President, a cordial vote of thanks to Prof. Darwin was passed.

The Liverpool Astronomical Society.—This Society held its fourth ordinary Meeting on Monday evening, January 4, at half-past seven o'clock, in the Royal Institution Rooms, Colquitt Street, Liverpool. Captain Jeffery, F.R. Met.Soc., occupied the Chair, in the absence of the President, who was on the Continent. The attendance was small, owing doubtless to the very severe weather. The papers will appear as usual in the Journal.

Arrangements have been made to place the Society's very valuable library in the Royal Institution; as yet, no definite arrangement has been made for the housing and use of the Rowlands Telescope.

We notice the recent death of John Hampden, a curious example of a man with a crank. In other matters of common life quite reasonable, he was converted by "Parallax" to the belief that the Earth was flat; and this he maintained with unflagging energy till his death. If ever a man consistently acted up to the motto "What I sez, I sez; and what I sez, I sticks to," it was Mr. Hampden; he not only said the Earth was flat, but proclaimed it, and wrote it; and abused others for not believing it; and swore it; and at last bet upon it, losing his wager; but still swore it. His language on the subject was powerful. He was one of those who regularly address the Astronomer Royal, and the most violent of them. There is another regular correspondent who believes himself tied to Satan for some indefinite period, and who graphically describes his experiences; but his command of extravaganza is as nothing to Mr. Hampden's. Peace to his uneasy soul

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The Council of the Royal Meteorological Society have arranged to hold, at 25 Great George Street, Westminster (by permission of the Council of the institution of Civil Engineers), on March 17 to 20 next, an Exhibition of Rain-Gauges, Evaporation-Gauges, Percolation-Gauges, and kindred Instruments. The Exhibition Committee invite co-operation, as they are anxious to obtain as large a collection as possible.

The Committee will also be glad to show any new Meteorological Instruments or apparatus invented, or first constructed, since last March; as well as Photographs and Drawings possessing Meteorological interest.

Persons willing to co-operate in the proposed Exhibition should furnish (not later than February 10) a list of the articles and an estimate of the space they will require to Mr. William Marriott, at 30 Great George Street, Westminster.

We are requested by the executors of the late Mr. Kullberg and by Mr. James Poole to contradict the rumour that Mr. Poole has taken over the business of Mr. Kullberg, which is being carried on by the executors.

Erratum.—No. 171, p. 64, line 6 from bottom, *for* "Fr. Hill's" *read* "Fr. Hell's."

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 173. MARCH. 1891.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, February 13, 1891.

Lieut.-Gen. J. F. TENNANT, C.I.E., R.E., F.R.S., *President*, in the Chair.

Secretaries: E. B. KNOBEL and A. M. W. DOWNING, M.A.

MR. DOWNING read the Minutes of the last Anniversary Meeting, which were confirmed.

Mr. Cottam read the Auditors' Report.

Mr. Knobel. On an examination of the accounts that have just been submitted, it will be found that the expenditure for the past year has exceeded the receipts by £134 13*s*. 7*d*. This is well and clearly explained by reason of the extensive alterations which we have made in the Library, and which have cost over £300—£249 12*s*. for alterations and £52 3*s* 10*d*. for furniture. I would desire to call the attention of Fellows to the new room at the top of the house, which now forms the New Library, and which I think is well worthy of their inspection. From another item in the account, it would appear that purchases for the Library have only amounted to £8 12*s*. 8*d*., but we have had during the past year a credit standing to us with a firm of booksellers, in consideration of some old non-astronomical books which we sold to them, and we have bought books to the extent of over £21 upon that credit, so that the amount of purchases for the Library is really £29 14*s*. 7*d*., and not £8 12*s*. 8*d*. as appears. It would have been difficult to have included this in the Treasurer's account, but the explanation is, I think, due from us to the Fellows.

The Secretaries then read the Report of the Council.

Mr. Woodd-Smith. I am very happy to move that the Report now read be received and adopted, and that it be printed and circulated in the usual manner, together with the Auditors' Report.

Mr. Perigal seconded the motion.

The Resolution was agreed to.

The President. The special business of this meeting is Mr. Chambers's proposition "that henceforth the Meetings of the Society be held at 5 P.M., and that Bye-law 44 be altered accordingly."

Mr. Chambers. I should not have troubled the Society with another motion on this subject so soon after the last, if you had not refused facilities for a poll of the members on the main question. It is the undoubted right of every Englishman and every lawfully corporate body to have a poll of absent members.

Mr. Turner. Sir, is this gentleman in order in questioning your ruling?

The President. I think Mr. Chambers is not in order in the the matter; besides that, I will point out, as a matter of fact, that it cannot possibly refer to a case like this. A poll could not be had at this Society, except by means which were unknown to the common law to which you refer.

Mr. Chambers. I don't wish to discuss that, but I referred to it to explain why I bring the motion up again. I have a perfect right to refer to it as a matter of fact.

Mr. Chambers then proceeded to state the arguments he made use of on previous occasions, quoting as precedents for the change the Royal Society and the British Astronomical Association. "Our meetings are attended by 50 or 60 members out of 600, and if the experiment of alteration to five o'clock meetings results in our attendance falling off very much, I shall be the first to acquiesce to the restoration of the old order of things."

Rev. A. Freeman. I beg leave to second Mr. Chambers's proposition. I do not care to appear in the character of a stormy petrel, but there is no reason why we should not try this change for a year, and it would certainly remove a good deal of objection on the part of those who feel that the evening meeting is highly inconvenient to them. Our Treasurer made a most important observation at the last discussion of this question, when he pointed out that to observing astronomers it was certainly a consideration to meet in the afternoon rather than the evening. A great number of our older members do not find it advisable to attend evening meetings and go home late, and I notice that at this afternoon meeting a good many are here who do not attend the evening meetings. Country members naturally feel that if they attend afternoon meetings they are able to go home the same night, whereas after the evening meetings they are not able to get home until 11 or 12 o'clock next day, and a half-day's work, especially on Saturdays, is a serious thing for them.

I think afternoon meetings would induce a more regular attendance of serious working astronomers. I do not mean the amateurs; I thought we were not a society of amateurs, but a society of professed astronomers. Do not consider only the interests of the

London members, the barristers and the business men who can conveniently attend in the evening, and who will not make any sacrifice whatever to oblige the country members and working astronomers, but give the project a fair trial for 12 months and see if the change of the time of meeting is advantageous to the Society.

Dr. Schuster. At this stage of the proceedings I should like to move an amendment which may meet the wishes of the two sides and may be of advantage to speakers who come after. I can speak in a very impartial way because both afternoon and evening meetings are equally inconvenient to me. The proposal I would make is to have meetings alternately one month in the evening and one month in the afternoon. You will soon find out which is best attended and can then decide the question, although you might find it an advantage to keep the proposed system up permanently. The proposal if adopted as an experiment does not commit you to so much as putting the meetings at once to the afternoon all the year round.

Sir Erasmus Ommaney. I rise to make an amendment. I am a great advocate for afternoon meetings and always have been. As a Londoner I should like the meetings at 4.30 instead of 5, and as we must allow 2 hours for the meeting, I think it would be better if we could get away at 6.30 instead of 7 o'clock. I propose that the time of meeting be altered to 4.30.

Mr. Knobel. It is impossible to take an amendment upon an amendment. One amendment should be disposed of before another amendment can be entertained.

Mr. Turner. If Dr. Schuster's amendment is in form I shall be happy to second it. I venture to think that the spirit of the original motion is preserved, in that the alteration in the time of the meeting must be a purely experimental one, and without treading upon the President's ruling we might make the alteration for a year. The main question was, I consider, legitimately considered at the December meeting. But we have in the present amendment a condition of things which gives us all we require, and must at the same time be only temporary. The question as to whether 5 o'clock or 8 o'clock is better for the Society I regard as essentially an experimental one. We have heard arguments for and against, and some of these cut both ways. Some section of the Society must be prevented from attending the meetings whatever time we finally adopt. Mr. Freeman pleaded for evening observers, but there are some professional astronomers who would find it an advantage to have a meeting which would prevent them having to observe. [Laughter.] There is a strong diversity of opinion about getting through Council work, but the Council themselves decided that they could get through their work. I think Dr. Schuster's amendment is an ingenious proposition to enable us to get the experiment made, therefore I second the amendment.

Mr. Chambers. My desire is to do the greatest good to the greatest number, and if the meeting decides for Dr. Schuster's amendment, I will willingly abandon my resolution and support the amendment.

Rev. C. J. Taylor. I am very much in favour of the second proposition before the Society to-day because it is a kind of compromise, and I hope will be satisfactory to a very large number of country members. As a country member I find evening meetings are very inconvenient to me, for I cannot get back home without some extra expense for hotel bills. The precedent of the young society, the British Astronomical Association, has already been brought forward by the mover of the resolution. I will take this opportunity of adducing a precedent from a Society not so young as the British Astronomical Association, and that is the Society for Psychical Research, for they have for some time held their meetings alternately in the afternoon and evening. I hope this meeting will be able to adopt some such practice.

Mr. Ranyard. There is some objection to holding the meetings at different times on different days. Many of us do not look at our cards and papers, and if we omit to do so we shall perhaps come at the wrong time. The plan might thus cause considerable inconvenience to some of us. I shall therefore vote against Dr. Schuster's amendment.

Mr. Knobel. I fully expected to hear from Mr. Chambers some reasons for bringing forward this resolution again after it had been disposed of at a previous Meeting. Perhaps, for the information of Fellows, it will be convenient to mention the history of the resolution. On May 8, 1885, after a Report from the Council of the result of the postcard series of questions which were issued, Mr. Chambers moved that the hour of meeting be 5 o'clock. That was put and lost. On Feb. 14, 1890, at the Annual Meeting, Mr. Chambers made the same proposition that he has made to-day, an amendment deferring the consideration of the subject being carried by 32 to 20. It was a proper thing after the deferring of the question that Mr. Chambers should bring it forward again; and he did in December last. On December 12, 1890, the definite proposition for 5 o'clock meetings was put and lost, the numbers being 18 for and 30 against. It was a singular thing that the moment that resolution, of which due and full notice had been given, was lost, Mr. Chambers should give a repetition of it in the same terms as before.

Mr. Chambers. Will you give the figures of the postcards in 1885?

Mr. Knobel. The figures of the postcards in 1885 were:—for 8 o'clock 106, for 5 o'clock 125, neutral 150, for other hours 17. It was the opinion of the Meeting in 1885 that no action should be taken on the postcards unless there was a decided majority one way or another, and that being the case no action was taken in the matter. Mr. Chambers, in 1885, moved his resolution, which was lost; and it met a similar fate in Feb. and Dec., 1890. I want to ask whether the motion to-day is not vexatious, and whether, if it is lost, Mr. Chambers will give notice to bring it forward in two months' time, and so on, every two months? A word is required to relieve us from anxiety on that point. Another point is, Mr. Freeman said we are not a Society of Amateurs. When I said we are a Society of Amateurs——

Mr. Freeman. I was not alluding to you, Mr. Knobel, but to some other Fellow of the Society.

Mr. Knobel. I beg your pardon. I had used the word, I believe, in its proper etymological meaning—that we are a Society who devote ourselves to Astronomy apart from any other consideration than the love of the science itself. I think the proper order now is that permission should be given to Dr. Schuster to propose his amendment before it can be submitted and discussed.

The Astronomer Royal. It is quite permissible for any two Fellows to propose and second an amendment without asking permission or putting it to the vote.

Mr. Knobel. The question was first raised when Lord Crawford was in the chair; and the practice was then instituted that an amendment could not be accepted without the Meeting gave permission. That principle was acted upon by Dr. Glaisher when he was in the chair, by the Astronomer Royal, and by our present President.

Mr. Turner. Are you not confusing *discussing* the question with *putting* it?

The President. I must say I am surprised at this objection. I should never have hesitated about it myself. It seems to me as plain as possible that an amendment rising out of a resolution can be made of right and without permission.

Mr. Ranyard. I think the usage is that the leave of the Meeting should be taken that the amendment become the substantive resolution; and if it is proposed to make it the substantive resolution, leave of the Meeting should be asked.

The President. I do not understand that is intended at present. If the amendment is carried the resolution will be abandoned, but not until then.

Mr. Chambers. Then I am willing to withdraw my resolution in favour of Dr. Schuster's amendment. With regard to what Mr. Knobel has said, I may explain that I brought forward this motion to-day because the President suggested that it was a much more proper one for discussion at an annual meeting than at an ordinary meeting.

A Member. I understand it is impossible for Mr. Chambers to withdraw according to Parliamentary practice. We are called here to discuss this particular question; and I do not see how an amendment concludes the discussion of the original motion.

The President. The original proposition cannot be withdrawn as a matter of right, but only by permission of the Meeting. An amendment can be made as a matter of right.

Mr. Common. You have already ruled that this amendment can be put.

Mr. Knobel. I am aware that the President has overridden the action of his predecessors.

Capt. Noble. I do not know if there is anything in the constitution of this Society which precludes such an amendment. I am chairman of several public bodies, and I never heard that such an amendment could not be put. Notice must be given of a resolution, but I can speak very confidently that any amendment can be made.

A Fellow. I think the amendment if carried will make confusion worse confounded. The original motion has been thoroughly thrashed out and decided against several times. The Council having put their objection aside, it must be shown that the change would be advantageous to the Society as a whole. The position has not altered in any way since Mr. Chambers's last motion. I was perfectly astonished when I saw the motion on the agenda. I have never seen anything so indelicate in any Society I have been connected with during 40 years of experience, as this matter being brought forward on Feb. 13 after being decided against for the third time in the preceding December. Last Annual General Meeting decided against this motion, so there is little validity in Mr. Chambers's plea that he wished the matter discussed at an Annual General Meeting. I do hope that after to-day we shall not hear of this matter for at least 12 months.

Mr. Brett. I do not think you can prevent us from bringing it forward again.

The President. We must leave that to the good taste of the people who bring it forward.

Mr. Brett. I want to make Mr. Knobel's mind easy. If this motion is rejected to-night, I intend to bring it forward myself on the next possible occasion. [Laughter.]

Mr. Freeman. I think Dr. Schuster has proposed a most judicious course. My idea is that it would be better to have meetings twice a month instead of once, and that one meeting should be in the afternoon and the other in the evening. That, however, is a large change, and would put the Council to a great deal of extra trouble; but Dr. Schuster's amendment does not do that.

Mr. J. G. Petrie. I came in late because I find these afternoon meetings are very difficult for me to attend. At the British Astronomical Association I have the greatest difficulty in getting there by 5 o'clock, and am satisfied if I get there by 6. That Society would, I believe, be prepared to change its meeting-time to a later hour. The Fellows of this Society have not yet come to the position of repose that many of the Fellows of the Royal Society have come to, and it would be a serious thing to change the hour of meetings as an experiment. The country members will not fill the blanks caused by those who cannot attend in the afternoon. All the country members do not desire the change, and one reverend gentleman at the last meeting assured me that he would prefer not to go home the same evening. Another thing is, we want a break between business engagements and these meetings. The *pabulum* supplied here is not of the very lightest description, and it would be hard to leave our books and papers to come here without that little break which is so necessary to the due enjoyment of these meetings.

Mr. Stone. Personally I am in favour of afternoon meetings; but we must consider the convenience of the greatest number. If all Fellows vote for their own personal convenience we shall get a proper decision. As for the mixed meetings, I cannot say I favour that proposal. When the circulars were sent round years ago to get the general sense of the Society the numbers were very equal, although many of the votes came from Fellows who were not likely to attend either meetings, and therefore ought not to have voted. I would prefer this question to be voted upon pure and simple; and if the advocates for early meetings are beaten they should accept their position.

Mr. Sydney Waters. As the proposer of the original resolution in 1885, I should like to say I am entirely in accord with Dr. Schuster's amendment. The best experiment we can make will be to allow a trial of the amendment to be made for a year.

A division was then taken, when 27 voted for the amendment and 37 against it. The amendment was declared lost.

The original motion was then put, and on division was lost, 31 voting for it and 35 against it.

The President appointed Mr. R. Inwards, Mr. J. H. Mitchener, and Mr. W. Schooling, Scrutineers of the Ballot for the election of Council and Officers for the ensuing year.

The following was the result of the Ballot:—

President: Lieut.-Gen. J. F. Tennant. *Vice-Presidents*: Capt. W. de W. Abney, Prof. J. C. Adams, Dr. J. W. L. Glaisher, and Mr. E. J. Stone. *Treasurer*: Mr. A. A. Common. *Secretaries*: Mr. A. M. W. Downing and Mr. E. B. Knobel. *Foreign Secretary*: Dr. W. Huggins. *Council*: Sir R. S. Ball, Prof. A. Cayley, Mr. W. H. M. Christie (Astronomer Royal), Hon. Sir James Cockle, Mr. George Knott, Mr. Frank McClean, Mr. E. W. Maunder, Capt. W. Noble, Mr. W. E. Plummer, Mr. Isaac Roberts, Dr. E. J. Spitta, and Mr. H. H. Turner.

Mr. W. H. Maw proposed and *Dr. Bryant* seconded a resolution "that the thanks of the Meeting be given to the President, Vice-Presidents, and other members of the Council who now retire, for their services in promoting the objects of the Society." This was carried unanimously.

On the motion of *Mr. E. J. Stone*, seconded by *Capt. Noble*, the thanks of the Meeting were given to the Scrutineers of the Ballot.

G. P. B. Hallowes, 77 Lansdowne Road, Didsbury, Manchester; *Shīn Hirayama*, Astronomer at Tokyō Observatory, Japan; *Robert*

John Pearce, M.A., D.C.L., Professor of Mathematics in Durham University; *John M. Stone*, St. John's College, Cambridge, were ballotted for and duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

John Billington Booth, J.P., Preston, Lancashire (proposed by Ralph Copeland); *Hannibal Mirabita*, 134 Strada Britannica, Alexandria, Egypt (proposed by Rev. Joseph Reed).

BRITISH ASTRONOMICAL ASSOCIATION.

The fourth ordinary Meeting of the above Association was held in Barnard's Inn Hall, Holborn, on Wednesday, Feb. 25, Captain Noble, F.R.A.S., the President, in the Chair.

Amongst the presentations announced were Mr. Cottam's two fine Star Atlases, presented by their designer, and a number of beautiful prints of stellar photographs from Mr. Roberts.

The General Secretary, Mr. P. F. Duke, read the names of 18 Candidates proposed for election.

Dr. Gill was called upon to give an account of his work at the Cape in connection with Stellar Photography. He said that Stellar Photography might be said to have commenced there with the photographing of the Great Comet of 1882. This was first seen by Mr. Finlay, and rapidly increased in brilliancy until it became one of the most sublime phenomena he had ever seen in the heavens. He was unfortunately absent from the Observatory on the day that it transited the Sun, but returning the next day was able to point it out shining in full daylight close to the Sun. A photographer from the town came up to the Observatory, his camera was strapped on to the telescope, and with an hour's exposure a photograph of the comet was obtained. The success of this experiment convinced him that this was the most satisfactory way to chart the heavens. The MM. Henri, at Paris, had carried out the same idea, but he had previously obtained a grant from the Royal Society to pay a photographer, in order to complete to the South Pole by photography the work which Argelander did for the northern heavens. The photographs had been taken, and Professor Kapteyn, of Gröningen, who had undertaken the measurement of the photographs, had completed about two thirds of the work. He described the new telescope recently provided for the Cape Observatory, and illustrated the various improvements introduced by Sir H. Grubb and himself. Although the telescope only arrived in August, about 400 photographs had been already secured for special investigations, in addition to experimental photographs for the approaching meeting in Paris of the International Committee.

Mr. Downing remarked that it had been stated that Dr. Gould had done Argelander's work for the South. This was not so. He yielded to no man in admiration of Dr. Gould, but he had not designed to extend Argelander's work. Dr. Gill's reconnaissance was essential for the proper carrying out of the International Survey.

Mr. Edwin Holmes read a paper on Mars.

Twenty-eight new members were ballotted for and elected. The Meeting was then made Special to discuss the proposed Rules for Branches, which were carried.

MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

THE usual monthly meeting of this Society was held on Wednesday evening, the 18th instant, at the Institution of Civil Engineers, 25 Great George Street, Westminster: Dr. C. T. Williams, M.A., Vice-President, in the Chair.

Mr. C. L. Brook; Mr. C. E. De Rance, F.G.S., Assoc. Inst. C.E.; Mr. J. Eden, Assoc. M. Inst. C.E.; Mr. J. C. Mundell; and Mr. J. Sidebottom, J.P., were elected Fellows of the Society.

The following papers were read:—

1. "The Great Frost of 1890–91," by Mr. C. Harding, F.R. Met. Soc. This paper dealt with the whole period of the frost from November 25th to January 22nd, and it was shown that over nearly the whole of the south-east of England the mean temperature for the 59 days was more than 2° below the freezing-point, whilst at seaside stations on the coast of Kent, Sussex, and Hampshire the mean was only 32°. In the extreme north of Scotland as well as in the west of Ireland the mean was 10° warmer than in the south-east of England. In the southern midlands and in parts of the south of England the mean temperature for the 59 days was more than 10° below the average; but in the north of England the deficiency did not amount to 5°, and in the extreme north of Scotland it was less than 1°. The lowest authentic reading in the screen was 0°·6 at Stokesay, in Shropshire, but almost equally low temperatures occurred at other periods of the frost. At many places in the south and south-west of England as well as in parts of Scotland and Ireland the greatest cold throughout the period occurred at the end of November; and at Waddon, in Surrey, the thermometer in the screen fell to 1°, a reading quite unprecedented at the close of the autumn. At Addington Hills, near Croydon, the shade thermometer was below the freezing-point each night, with one exception, and there were only two exceptions at Cambridge and Reading; whilst in the Shetlands there were only 9 nights with frost, although at Biarritz frost occurred on 31 nights, and at Rome on 6 nights. At many places in England the frost was continuous night and day for 25 days, but at coast stations in the north of Scotland it in no case

lasted throughout the 24 hours. On the coast of Sussex the temperature of the sea was about 14° warmer than the air throughout December, but on the Yorkshire coast it was only 6° warmer and in the Shetlands and on parts of the Irish coast it was only 3° warmer. The Thames water off Deptford, at 2 feet below the surface, was continuously below 34° from December 23rd to January 23rd, a period of 35 days, whilst the river was blocked with ice during the greater part of this time. In Regent's Park, where skating continued uninterruptedly for 43 days, the ice attained the thickness of over 9 inches. The frost did not penetrate to the depth of 2 feet below the surface of the ground in any part of England; but in many places, especially in the south and east, the ground was frozen for several days at the depth of one foot, and at 6 inches it was frozen for upwards of a month. In the neighbourhood of London the cold was more prolonged than in any previous frost during the last 100 years, the next longest spell being 52 days in the winter of 1794-5, whilst in 1838 frost lasted for 50 days, and in 1788-9 for 49 days.

2. "The problem of Probable Error as applied to Meteorology," by Mr. T. W. Backhouse.

MEETING OF THE ASTRONOMICAL SOCIETY OF THE PACIFIC.

1891, January 31.

THE Society met at 408 California Street. In the absence of President Holden, Vice-President Pierson presided.

At the meeting of the Directors, held prior to the regular meeting, 55 gentlemen and ladies were elected, among them being Messrs. Ellery and White, of the Melbourne Observatory.

An important paragraph was added to the Bye-Laws, by which groups of members in any city, excepting San Francisco, are authorized to form sections, to be governed by local boards of officers and rules. The proceedings of such sections may be printed in the regular Publications of the Society. No person shall be eligible to membership in a section without being a member in good standing in the parent Society.

Under the provisions of this amendment a Chicago Section has been formed, and its existence recognized by the parent Society; and has elected Officers and an Executive Commitee as follows: G. A. Douglas, Chairman; G. E. Hale, Secretary; and R. W. Pike, Prof. C. B. Thwing, and M. D. Ewell.

The Directors adopted a seal for the Society, a cut of which will appear in the next publication.

It was also resolved that, beginning with the present year, all moneys received from life membership fees should be invested in a separate fund, and only interest therefrom to be used in defraying the expenses of the Society.

Messrs. Schaeberle and Burckhalter were reappointed to act as a Committee on Comet Medal for the year 1891.

At the Regular Meeting, held after the Directors' Meeting, a Committee consisting of Messrs. Von Gelden, Theat, Dewey, Ewer, and Lowden was appointed to nominate a ticket of eleven names for the next Board of Directors, and three names for Committee on Publications, to report at the next Meeting (March 28th), which will be the Annual Meeting. Messrs. Ziel, Johnson, and Leuschner were appointed to make the annual examination of the Accounts of the Treasurer.

The following papers were presented to the Meeting:—

a. "The Carleton College Observatory," by the Director, Prof. W. W. Payne.

b. "The August Meteors," by W. H. S. Monck, Dublin, Ireland.

c. "Corrections to Watson's Theoretical Astronomy," by W. W. Campbell, Ann Arbor, Michigan.

d. "Notes on Dark Transits of Jupiter's Satellites," by John Tebbut, Windsor, N.S.W.

The Meeting then adjourned.

CHARLES BURCKHALTER, *Secretary.*

The Origin of the Stellar Systems.

IN the February number of the 'Observatory' I have pointed out what I am convinced is the physical meaning of the high excentricities of the stellar orbits. The mathematical investigation therein referred to seems to show that tidal friction is an amply sufficient cause to account for the great excentricities, but to complete the syllogism we ought to show that no other possible cause can be assigned as the source of the observed phenomena. The array of multitudinous resources revealed in the Cosmos is such that in general it is difficult to set rigorous bounds to the possibilities of Nature. Fortunately, however, in this particular case the circumstances surrounding the question at issue are such as to justify the belief that in general no other cause than tidal friction can be assigned to account for the excentricities observed in the binary orbits. Some astronomers may conceive that the excentricities indicate the fortuitous origin of these systems from separate stars. A little reflection, however, will make it clear that such an hypothesis is untenable. For such a chance orbit would be in general hyperbolic; and it is easy to show that should the orbit in some cases even be elliptic, its absolute dimensions would very far exceed those of binary orbits hitherto computed. Prof. Lockyer, in his 'Meteoric Hypothesis,' has suggested that double stars originate through the approach of separate swarms of meteorites. But this suggestion to my mind is even more unsatisfactory than the foregoing, since it not only encounters the same difficulties, but must in addition explain how the smaller swarm would escape disintegration under the action of the chief swarm. For the smaller swarm would enter on an orbit highly elliptic or hyperbolic. In case the orbit is elliptic, the disintegrating action of the larger swarm, in a comparatively short time, will scatter the meteorites of the smaller swarm along the whole length of its orbit—reduce it (as the Sun reduces a comet) to a train of meteoric dust. Moreover, the observed excentricities of the stellar orbits are in general hardly sufficiently high to support this hypothesis.

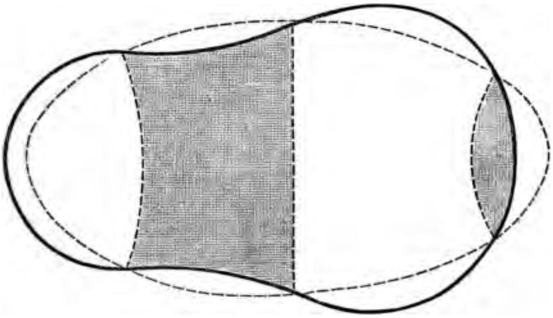
Again, it has been suggested that an explosion in the process of separation has rendered the orbit of the detached body so highly excentric. This suggestion of explosions is purely an hypothesis, and we are not able to affirm that such phenomena really take place, except probably in our Sun. But granting that they really take place in nebulæ, it seems very difficult to see how such explosive action could divide a nebula into two nearly equal masses. It seems more probable that the meteorites of a nebula under such conditions would merely be scattered in every direction, and that the parts would almost immediately fall back together. But even should the detached parts not immediately reunite to the largest remaining body they would revolve around it as meteoric swarms in excentric orbits, and it is impossible to conceive how, under the disintegrating action of the chief mass, they could ever be gathered into individual small bodies; much less could they be agglomerated into a second large mass.

All the foregoing hypotheses seem, therefore, to encounter fatal objections. I will now venture an hypothesis which demands nothing extraordinary, and rests, moreover, on a dynamical basis.

A homogeneous fluid sphere devoid of rotation is always in equilibrium, however its particles may be arranged with respect to one another. If this sphere be endowed with a slow rotation about any axis, it becomes a spheroid; here again we may suppose the particles exchanged with one another without any disturbance of the equilibrium. We may regard this homogeneous spheroid as made up of successive infinitely thin layers, all having the same ellipticity. If the rotation of the spheroid is quickened its ellipticity increases, and the ellipticities of all the strata increase with equal rapidity. The particles of a heterogeneous fluid sphere devoid of rotation are in stable equilibrium when, and only when, they are so arranged that the density decreases from the centre to the surface. Such a sphere is composed of successive infinitely thin layers of a uniform density, but the layers decrease in density from the centre to the surface. If now the sphere be endowed with a slow rotation, it passes into a spheroid, and all its layers become spheroidal. But in this case of heterogeneity, the layers do not, as in the case of homogeneity, retain the same ellipticity in passing from the centre to the surface. The ellipticities increase, as is mathematically established by Laplace in the second volume of the 'Mécanique Céleste.' Thus the ellipticities of the layers of a heterogeneous fluid spheroid in equilibrium follow a law the reverse of the densities. Sir John Herschel has observed that certain nebulæ actually appear to confirm this law. Since the ellipticities were zero when the body was devoid of rotation, it is clear that acceleration of axial velocity will cause them all to increase until a certain limit is reached. But all the ellipticities will not increase with equal rapidity; the ellipticities of the outer, less dense, layers will gain ever more and more rapidly upon the denser and less elliptic layers at the centre. The divergence in the ellipticities thus increases as the rotation proceeds. With a given velocity of rotation the divergence increases with increase of heterogeneity. This being established, let us now consider some other figures of equilibrium. Throughout this discussion we are supposing the law of attraction to be Newtonian gravitation. In the figures of equilibrium hitherto determined, owing to mathematical difficulties at present insurmountable, nearly all investigators have presupposed the fluid to be homogeneous. Among the figures established on this hypothesis is the Jacobian ellipsoid of three unequal axes; and the spheroid may be regarded as a particular case where two of the axes of the ellipsoid are equal. This Jacobian ellipsoid set rotating in equilibrium about its shortest axis is rigorously stable.

M. Poincaré has discussed the dynamics of the Jacobian ellipsoid with particular care ('Acta Math.' vol. vii.). He finds that as the rotational velocity of the ellipsoid increases the ellipsoid gradually flattens out, and finally passes into the accompanying very remarkable figure.

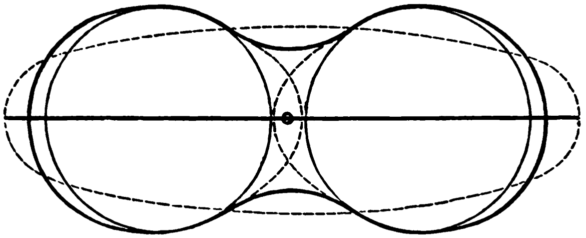
Fig. 1.



Section of M. Poincaré's figure of equilibrium. The shaded portion falls within the ellipsoid.

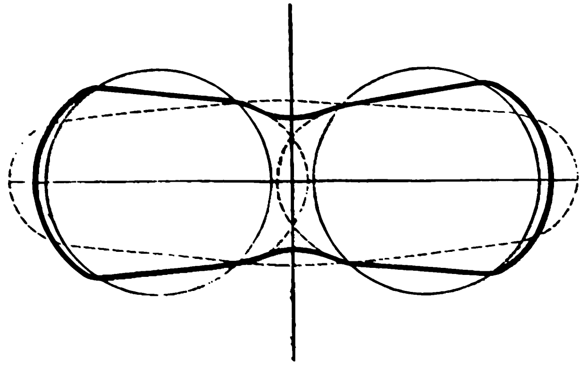
Fig. 1 represents a section of M. Poincaré's figure through the shortest and longest axes (corresponding respectively to similar axes in the ellipsoid). This furrowing of the Jacobian ellipsoid parallel to its smallest section indicates, as M. Poincaré points out, a tendency of the body to split up into two comparable masses. At the stage represented in Fig. 1, the equilibrium is rigorously stable. M. Poincaré expresses disappointment in the conclusion of his excellent research that his investigation did not throw much light upon Laplace's 'Nebular Hypothesis.' Prof. Darwin undertook a somewhat similar investigation independently; and completed it after the publication of M. Poincaré's paper. He sought to determine the figure which two homogeneous spherical fluid masses would assume under their mutual attraction when set in synchronistic orbital motion about their common centre of gravity so close together that their surfaces nearly touched. His result is given in Fig. 2 for equal masses.

Fig. 2 (*a*).



Section (perpendicular to axis of rotation) of Mr. Darwin's dumb-bell figure of equilibrium. Dotted section Jacobian ellipsoid of same mass and moment of momentum. Scale one-third of the original.

Fig. 2 (*b*).



Section through axis of rotation.

In Fig. 2 the whole sections are given—one through the axis of rotation, the other perpendicular to it. For details and an admirable discussion of this remarkable dumb-bell figure of equilibrium, see 'Phil. Trans.' 1887.

At the conclusion of his work, Mr. Darwin, like M. Poincaré, remarks that but little help is gained towards understanding Laplace's 'Hypothesis.' He adds, that both his results and those of M. Poincaré seem to show that the mass separated should bear a very much larger ratio to the parent mass than is observed in the planets and satellites of the solar system. He states that the mass separated cannot bear a less ratio (in case of homogeneity) than about one-thirtieth. He agrees with M. Poincaré that when the equilibrium of the Jacobian ellipsoid breaks down, the body splits up into two comparable masses.

Now let us examine the significance of these mathematical results in our study of the origin of the binary stars. Before we proceed further let the reader compare the above figures with Sir John Herschel's drawing of Double Nebulæ, published in 'Phil. Trans.' for 1833. Compare M. Poincaré's figures with those numbered by Sir John Herschel respectively 68, 69, 70, 71, 72, 73, 74 (representing the nebulæ numbered 1252, 1202, 604, 1146, 444, 2197, 1408); and I think it will be impossible to escape the conclusion that the foregoing mathematical results are exemplified in the heavens. Nebulæ absolutely homogeneous of course do not exist; yet there are abundant reasons for thinking that in many cases the departure from homogeneity is not very marked. If the nebula is not heterogeneous to a considerable degree, we may take the figures of M. Poincaré and Prof. Darwin to represent substantially what would occur as gravitational contraction proceeds. And we may consider the figure of a nebula undergoing development momentarily, but not secularly, stable, owing to the continual dissipation of its energy and the contraction consequent therefrom. Hence we see that if the nebula be homogeneous the mass-ratio, which will obtain between its parts when it splits up, will be a very large fraction, perhaps one-half, one-third, or one-fourth, or smaller, and possibly, under certain conditions, unity. The above mathematical investigations seem to show that in general the masses would not be exactly equal, but always comparable—*i. e.* always of the same *order*. If the heterogeneity be not very considerable, when the parent nebula splits up, this same relative mass-ratio will be nearly maintained. There is reason to believe that many of the nebulæ are not very heterogeneous, and we ought therefore to expect a separation into comparable masses. This theoretic inference accords with the great number of double nebulæ and double stars actually observed in the heavens. The greater the heterogeneity the greater the divergence in mass-ratio from that obtaining in case of homogeneity. This clearly follows from what we have said elsewhere concerning the concentration of density about the centre of the parent nebula, and the increase in the ellipticities of the strata as we pass to the surface. For the part thrown off will be taken chiefly from the peripheral regions of the parent mass; and even should the volume detached retain the same ratio to the parent volume, as in the case of homogeneity, still the mass would bear a much smaller ratio to the parent mass, owing to the inferior density of the part detached. But, in fact, it is easy to see that both the mass and the volume split off will be less in case of heterogeneity. If the nebula be very heterogeneous, the parts detached ought to bear a very small mass-ratio to the central body. With respect to the planetary system this conclusion would seem to indicate that when the planets were formed our original nebula was very heterogeneous. Under this hypothesis our remarkable system is seen to be a particular case—an exception to the general process of cosmic evolution, by which nebulæ develop first into double nebulæ, and then into systems of binary stars. Thus far the mass-ratio of the components of stellar systems has been determined satisfactorily from measurements in but few cases. But if we take the binary stars as a *class* we shall not err greatly in supposing the mass-ratio to correspond with the amount of light we receive, and this can be determined from the magnitudes of the stars.

In particular cases this method of inferring the relative masses of stars of a system from their magnitudes would doubtless fail. And in general the value thus deduced would probably be a little too small; still, it would at least give a rough approximation to the truth. It now remains to examine the orbit which the detached mass would pursue when first split off. From the very nature of nebular contraction we may with safety assume that acceleration in angular velocity proceeds slowly and steadily—there are no sudden jerks or stops. This being the case, it is certain that separation takes place very slowly; and we may unhesitatingly affirm that in general many revolutions are performed while the splitting process is in progress. Sudden accelerations in angular velocity being impossible, we see that the orbit the detached mass begins to pursue must be nearly circular. Were the orbit highly excentric, the action of the larger body would soon reduce the smaller mass to a train of meteoric dust; and, when once strewn out into a girdle, it could never condense into a star. But the mass split off being set revolving in an orbit nearly circular, the action of the larger body could not spread the smaller swarm out into a meteoric ring; and, condensation forthwith proceeding, it would escape disintegration when the orbit eventually became expanded and highly excentric from the secular effects of tidal friction. When just separated (as is apparently the case in Herschel's figure 68) the masses doubtless rotate and revolve in synchronism. But the dissipation of the energy of either mass soon permits gravitational contraction, and this causes the axial angular velocity to surpass the orbital angular velocity, and then tidal friction begins to operate. Under these conditions tidal friction increases both the major axis and the excentricity of the orbit. And in general these elements will continue to increase until the rotations of both stars are reduced to synchronism with the orbital revolution. When this state is reached, we have the maximum of excentricity; afterwards the excentricity is again very slowly reduced to zero by the secular action of libration. Such a rigid state would probably not be reached until both bodies have become entirely dark, since the angular velocity will continue to accelerate as long as contraction takes place. When finally this state is reached, however, the stars will show always the same faces to each other, and the system will continue to revolve as though rigidly connected for ever, unless acted upon by external forces, or ethereal or meteoric resistance. These suggestions of secular dynamic instability have a particular interest also when applied to systems of the Algol type, where the bodies are not only both very large, but also very close together, and therefore the tides in all probability enormous. From what we have said it will appear that astronomers have hitherto greatly underestimated the importance of tidal friction as a formative agency in the history of the Universe. It has certainly left a distinct impress upon the orbits of the stellar systems, and even in the case of the solar system it has exercised some influence. I do not, therefore, hesitate to rank tidal friction as one of the most important agencies operative in the development of the various systems of the Cosmos.

In conclusion, I beg to add that this and my former article constitute only a very brief outline of the general conclusions at which I have arrived by the investigation mentioned—an investigation which explains satisfactorily the great excentricities of the stellar orbits, the large mass-ratio of the components, and intimately connects binary systems with double nebulæ, constituting therefore a coherent theory, of which the probability, to my mind, closely approaches certainty.

It is difficult to see how either M. Poincaré or Prof. Darwin could have felt disappointed, had he realized the full significance of his splendid dynamical researches as applied in the Universe at large, and not merely as reflected by the solar system. It appears to me that more light will be thrown upon the general process of cosmic development, if, instead of studying the Universe in general through the solar system, we base our general deductions on a study of the double nebulæ and stellar systems. Thus we may hope eventually not only to attain to a safer scientific basis for stellar cosmogony, but we may also even approach the formation of the solar system from a new point of view—from the sidereal heavens. On first sight, it appears doubtful whether the planets and satellites were separated, as Laplace supposed, in the form of rings; a separation in the form of lumps or bulky masses seems in general more probable. Finally, whatever may prove to be the value of the ideas I have herewith advanced, it is but proper to state that whilst I began the investigation and arrived at the main conclusions independently, nevertheless, in the exact mathematical investigation I have profited by a study of Prof. Darwin's extensive tidal researches; and therefore I should be ungracious, if not indeed in some measure unjust, if I did not acknowledge my special indebtedness to Prof. Darwin for some methods of research employed in his very able papers. T. J. J. SEE.

University, Berlin, Prussia, 1891, Feb. 7.

John Harrison.

ON the 24th of this month, 115 years will have elapsed since John Harrison departed this life, at the advanced age of 83; and although the centenary of his death was not noticed in scientific circles, it certainly was of sufficient importance to have been thus distinguished. There is thus the more reason for recalling some of the more interesting facts about him on the present occasion, when several of his productions are about to be forwarded from the Royal Observatory to the approaching Royal Naval Exhibition at Chelsea. It is not my intention to attempt to give anything like a biography of Harrison, but to confine myself to the history and performance of the celebrated instrument, for which he received the Government premium of £20,000 offered in the year 1714. Often as this instrument has been written about, it has never yet been quite correctly described, from the fact that most writers have not seen it, and others have not had the opportunity of closely inspecting it. Having recently had it entrusted by the present Astronomer Royal to my care for the purpose of cleaning and examination, I feel in a position to write with some confidence on the subject, and also think that I can explain to a considerable extent the reason of the very great unpleasantness which took place between Harrison, the Board of Longitude, and the Astronomer Royal of the time.

The instrument is known as Harrison's fourth timekeeper (some writers have erroneously styled it the third); it is the first which he made in a portable form, the others being more in the form of clocks, in cases some 2 feet square. It is contained in a double silver case 6 inches in diameter, and, although popularly styled a "chronometer," is really only a verge watch. The seconds' hand is in the centre, and, from the construction of the escapement, moves in a tremulous manner, which is not convenient for observation. Modern chronometers beat half-seconds.

The three chief features in the instrument are the going fusee, the compensation curb, and the remontoire train. The importance of the first two cannot be over estimated, but the value of the last is doubtful, although it appears to have been the leading idea in Harrison's mind.

Looking back on his work, we consider in these days that his escapement was on a wrong principle, and that he started from a bad foundation in using it, apart from the difficulty of making it properly. It is affected, more than any other, by inequalities in the power of the mainspring, so he added the secondary force of the remontoire, in order to communicate a constant impetus to the escapement at intervals of seven and a half seconds. The ingenuity displayed in the arrangement is simply marvellous, but so much extra friction is introduced by the addition of the remontoire train that it is doubtful whether the extra force (which is imparted by the alternate coiling and uncoiling of a spring) more than counterbalances it. All the acting parts of the instrument are closely concealed by ornamental brass work, so that it is impossible to examine them without partly taking it to pieces, and I venture to assume that this was one cause why the Board of Longitude were prejudiced against the instrument when it first came before them. I must not omit to state, however, that the workmanship is perfect throughout, the wheels and pinions are beautifully cut, and the steel has been hardened and tempered in the most scientific manner possible, otherwise the pivots and springs would not stand the manipulation which is necessary to get them in their places. High polish has not been aimed at, but everything has been done with a view to utility. On the plate is engraved "John Harrison & Son, 1759."

In 1761 the instrument was sent for trial in a King's ship on a voyage to Jamaica and back, under the charge of Harrison's son, and the longitude having been determined within 18 miles (the maximum condition being 30), Harrison claimed the reward, but the Board of Longitude considered that further trials should be made. On August 17, 1762, Harrison was asked if he were willing that it should be tested at the Royal Observatory under the supervision of Mr. Bliss, the newly appointed Astronomer Royal, but he decidedly objected to let it go out of his hands.

Finally, it was arranged that it should be sent on another voyage to the West Indies, and that he should previously deliver a sealed up rate to the Secretary of the Admiralty. This he did on March 26, 1764. Dr. Maskelyne, who the following year became Astronomer Royal, went out in the ship in order to observe the daily performance of the instrument, which performed admirably, and determined the longitude within 10 miles. The first half of the reward was then paid, but further conditions were imposed before the whole was promised—one being, that an exact duplicate must be made by some one else. This task was accomplished by Larcum Kendall, Harrison's apprentice, who took higher ground with the Admiralty than chronometer makers are able to do now, as his stipulated price was £400, payable in advance, and he only undertook to make exact copies of the various parts without being held responsible for the performance of the instrument. This of course did not tend to make Harrison's production any more popular, but he said himself that he expected the cost in a few years would not exceed £100. It has been erroneously stated by several writers that this copy, by Larcum Kendall, was used by Captain Cook on his second and third voyages, the fact being that it has never seen actual service like the original. Captain Cook's—made by Larcum Kendall in 1771—is constructed on much simpler principles, and has no remontoire action. I was entrusted with the task of cleaning it at the end of 1889 by the Council of the Royal United Service Institution. Unfortunately, the escapement is worn out. But to return to Harrison's. On the 5th of May, 1766, it was handed over by the Lords of the Admiralty to the custody of the Astronomer Royal, for the purpose of being tested at the Royal Observatory for a period of ten months, viz., to March 4, 1767. It fully sustained the reputation which it had gained on the West India voyages, as its rate in ordinary temperatures was exceedingly uniform—the first day of rating it gained 17·6, and the last 18·7; but when the temperature fell to 26° it gained nothing, which, unfortunately for Harrison, was the opposite to what he said it would do. It is a proof, however, that he had so far mastered the principles of compensation as to actually reverse the usual effects of cold on the going of a watch. The Astronomer Royal finally reported on the instrument as follows:—"That Mr. Harrison's watch cannot be depended upon to keep the longitude within a degree in a West India voyage of 6 weeks, nor to keep the longitude within half a degree for more than a fortnight, and then it must be kept in a place where the thermometer is always some degrees above freezing: that in case the cold amounts to freezing, the watch cannot be depended upon to keep the longitude within half a degree for more than a few days, and perhaps not so long, if the cold be very intense: nevertheless, that it is a useful invention, and, in conjunction with the observations of the distance of the moon from the sun and the fixed stars, may be of considerable advantage to Navigation." The Astronomer Royal also took exception to the great increase of rate since the voyage; in the interval the instrument had been cleaned, and he considered that the former rate should have been maintained. Harrison was requested by the Board of Longitude to make two similar instruments for testing at the Royal Observatory, but it does not appear that this condition was complied with. He made one for the King, which after ten weeks' trial at his private observatory at Richmond, is said to have varied only 4½ seconds. It is currently believed that His Majesty personally interested himself on Harrison's behalf, and that the remainder of the reward was paid over in consequence of his intervention. As a matter of fact, the instruments had become obsolete before they were paid for. Mudge, Arnold, and Earnshaw were all in the field, appropriating Harrison's and each other's improvements indiscriminately.

When Captain Cook sailed on his second expedition in 1772, he had three of Arnold's with him, in addition to the historic one of Kendall's already mentioned. Two of these are in the possession of the Royal Society, and I cleaned and restored them early last year. They have a rude form of the present chronometer escapement and beat half-seconds. Although roughly constructed, they prove Arnold's inventive genius to have been quite on a par with Harrison's, and there is no doubt but that the Astronomer Royal and the scientific men whom he consulted considered that Arnold's instruments were likely in the long run to give better general results than those of Harrison and Mudge—the latter having adopted a form of the verge escapement which was quite as complicated as that of Harrison's remontoire. John Harrison, however, was the pioneer of the improvements which have gradually led up to the perfection which is expected at the present time. Five miles is now the maximum limit of error which is allowed in a first-class Government chronometer, and the longitude is frequently determined within one. At the same time, the science of navigation has made such progress that the manufacture of chronometers is yearly becoming less advantageous to those engaged in it; and it is only when a vessel, like the 'Calliope,' is suddenly driven out of her course, that those in charge of her feel really dependent on the excellence of their chronometers; but it is a sound principle to be always ready for the unexpected, therefore their use, in a limited degree, will always remain as a necessity.

JAMES U. POOLE.

Selenographical Notes.

JULIUS CÆSAR.—There are numerous points of interest in connection with this abnormally-shaped walled plain which render it well worthy of examination under many different phases of illumination, but especially so when it is on, or not far removed from, the morning terminator. Like Fracastorius and some other formations a wide section of the border is to a great extent wanting; but in this case the breach of continuity is less obvious, and what represents a southern wall is more or less easily traceable even under a moderately high Sun.

The most striking feature at sunrise is the wide deep valley running on the west side of the formation. Commencing a short distance north of Sosigenes, and gradually increasing in width as it proceeds northwards, it ultimately trends towards the Mare Vaporum, as do also numbers of hills parallel to its general direction. It is crossed transversely by three or four curved ridges which, though obscured at sunrise by shadow, are very distinctly seen about two days after the 1st quarter. Their appearance suggests that this great longitudinal hollow represents a row of large depressions, the division walls of which have been partially broken down. Schmidt's drawing also confirms this idea. The very remarkable character of the south border of Julius Cæsar is well seen when the north-east wall is on the morning terminator.

Under these conditions a view is obtained of very noteworthy details a little east of Sosigenes which seem to point very clearly to alluvial (or diluvial) action. Here, two curved valleys, separated by gently-sloping hills following the same curvature, and bending towards the north-west, may be traced nearly up to the southern end of the deep chasm just referred to, which exhibits a slight bend towards them or towards the south-east. Under high powers and good definition these valleys strongly impress one as being due to the action of liquid flowing from the eastward into the southern end of the great depression. The spurs from an apparent accumulation of material south of the valleys also exhibit clear indications of a drift towards the same point. As regards their gentle swelling form and intervening hollows, they remind one of the "coombs" in the chalk districts of Herts and Sussex. Schmidt shows, but far too coarsely, much detail in this region, but no idea of its actual character is to be gathered from his map or from those of Mädler and Neison. Sosigenes and Sosigenes *a* are connected by a wide low bank, which seems to have had the effect of diverting the drift of the liquid material towards the north-west into the upper end of the great valley. Altogether it is a district of great interest which will repay careful examination. There is a bright deep crater of considerable size on the crest of the north wall of Julius Cæsar, which is shown only by Schmidt, who also draws a small companion to it on the south, though it is especially prominent when the formation is on the morning terminator, the more so from its connection with the narrow culminating ridge of the eastern wall, which at this time is a very brilliant feature, extending nearly up to the more northerly of the two large depressions at the southern end of it. Neither Mädler or Neison show these depressions, each of which is as fully as large as Sosigenes on the opposite side of the ring, but very much shallower and of a different type, though they are prominently visible a day or two after sunrise. At an earlier stage they are obscured by the shadow of the wall, and only a small crater on the south border, west of the pair, is to be seen in the vicinity. On the western side of the floor and bordering the long deep valley is a mass of material of the same tint as the northern part of the interior, resembling a large mound heaped up against the wall. It is apparently about 14 or 15 miles in width on the south, where it abuts on the most northerly of the curved hills associated with the valleys mentioned above, and, gradually diminishing in breadth, dies out before reaching the north-western side of the floor. It is of no great altitude, though at sunrise a very distinct but narrow band of black shadow fringes it, and there is a shallow oval depression on its surface which cannot be regarded as a true crater. With this exception it is devoid of detail, and appears to be of a similar character to the large circular swellings on the north border of Fracastorius.

In the maps of Mädler and Neison the floor of Julius Cæsar is a blank as regards detail, and Schmidt shows only a long ridge running in a north to south direction near the centre and two still longer ridges (close together) between this and the west wall. The only objects I have detected are two low circular mounds, one nearly central, the other some distance west of it, and a doubtful object, bounded by an ill-defined black shadow at sunrise, near the foot of the north-east wall. This part of the interior is so dark under a low Sun that it is very difficult to decide as to the nature of the detail observed thereon, but this object has been suspected more than once to be a crater-cone from the form of its shadow. On January 16, 6^h to 7^h, the various features just described were well seen with a power of 350 on my 8½-in. Calver reflector, definition being very good, and in addition the delicate cleft running on the east side of Maclear up to Sosigenes *a*. There are two others west of this, which I have seen on previous occasions, that I failed to detect. Four or five small bright isolated mountains stand south of the low south border of Julius Cæsar which are not clearly traceable in the maps, one of the brightest and loftiest standing a short distance due east of Sosigenes near the eastern end of the most southerly of the shallow curved valleys described as trending towards the south end of the great longitudinal depression on the west of the formation. The monotonous floor of Julius Cæsar has a general slope towards the north, which may account for the difference, amounting to 1½° on the accepted scale of tints, between the darker northern end and the area south of the centre, which Neison estimates to be 3°. A commencement was made some years ago, under the direction of Birt, with observations intended to test the constancy of the tone of the floor under various solar altitudes, but they were never completed. In the case of the formation Boscovich on the east, the interior of which shows differences of tint very similar, Birt found the dark northern area almost invariable under all conditions, while the lighter remainder varied from 3¾° under a low Sun to 2½° under a high solar altitude. It would be an advantage to selenography if these tint observations, undertaken nearly twenty years ago, were resumed and diligently carried out under better optical conditions.

Thos. Gwyn Elger.

Beaumont House, Shakespeare Road,
Bedford, 1891, Feb. 19.

The Winter of 1890–1891.

In our January number we gave some particulars of the cold experienced to the end of the year as recorded at the Royal Observatory, Greenwich. We now append an account of the whole period of cold. It commenced on November 25, and until January 22 the mean temperature was below the average on every day excepting January 13, when it just reached the average.

Grouping the interval from November 19 to January 27, which includes the whole period of cold, into five-daily periods, we obtain the following results, to which are added, for comparison, the corresponding mean temperatures for Paris, as deduced from the numbers given in M. Flammarion's 'L'Astronomie' for the month of February.

1890–91.	Greenwich.					Paris.		Greenwich above Paris. Mean Temperature.
	Mean Maximum.	Mean Minimum.	Mean Temperature.	Departure from average.	Below 32°.	Mean Temperature.	Below 32°.	
	°	°	°	°	°	°	°	°
Nov. 19–23	54·8	46·6	51·3	+10·1	...	...	...	...
24–28	37·4	26·5	31·3	− 9·6	0·7	...	...	...
29–Dec. 3	37·0	25·8	32·4	− 9·2	...	25·0	7·0	+7·4
Dec. 4– 8	39·2	33·7	36·6	− 6·0	...	32·7	...	+3·9
9–13	32·3	25·2	29·0	−13·4	3·0	23·2	8·8	+5·8
14–18	30·3	22·9	27·1	−13·7	4·9	19·2	12·8	+7·9
19–23	30·9	19·1	26·2	−13·4	5·8	33·3	...	−7·1
24–28	32·8	25·3	29·9	− 9·2	2·1	24·6	7·4	+5·3
29–Jan. 2	31·6	23·4	27·8	−10·5	4·2	25·7	6·3	+2·1
Jan. 3– 7	33·9	26·1	30·3	− 7·3	1·7	28·9	3·1	+1·4
8–12	31·3	18·2	25·4	−12·4	6·6	21·6	10·4	+3·8
13–17	36·4	28·7	32·1	− 6·3	...	28·0	4·0	+4·1
18–22	36·6	26·3	31·9	− 7·2	0·1	26·2	5·8	+5·7
23–27	47·5	36·6	42·5	+ 2·7	...	...	...	...

These results bring out the persistent character of the frost, the mean temperature at Greenwich having been below the freezing-point for seven consecutive five-daily periods. The numbers in the last column show also the much greater severity of the cold at Paris, with the exception of one period December 19 to 23, when the ordinary condition of things became reversed. In Scotland the temperature was generally higher than at Greenwich throughout, frequently much higher, and thus in striking contrast with the Paris temperatures.

At Greenwich the temperature was continuously below 32°, on November 27, 28; December 10 to 19, 22, 23, 25, 28 to 30; January 2, 6 to 8, 10, 11, and 17 to 19; and indeed was continuously below 32° or fell below 32° at some part of the day or every day between November 25 and January 22, excepting December 3 to 8, January 4, 13, and 14. The greatest deviation of the mean daily temperature from the average in each of the three months was −10°·1 on November 28, −20°·7 on December 22, and −19°·3 on January 10; and the lowest temperatures recorded were 18°·3 on November 28, 13°·4 on December 22, and 12°·0 on January 10.

The mean temperature of the month of December was 29°·8, or 11°·0 below the average, which is much lower than that of any other December since the establishment of the Magnetical and Meteorological Observatory in the year 1841. The lowest previous means were 33°·0 in 1844, 32°·9 in 1846, 34°·0 in 1853, 33°·6 in 1870, 33°·2 in 1874, 33°·8 in 1878, and 32°·4 in 1879. At Paris the mean temperature of December was only 25°·9. W. E.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

John Fabricius and the Solar Spots.

Gentlemen,—

You refer in your last Number (p. 102) to the proposed erection of a monument in memory of David Fabricius, to whom we owe the first discovery of the variability of Mira Ceti and other interesting astronomical observations. I should like, with your permission, to say a few words about his son, John Fabricius, the author of the tract 'De Maculis in Sole observatis et apparente earum cum Sole conversione Narratio,' which was published in 1611, and contains the earliest observations of solar spots indicating by their motion a rotation of the Sun on its axis.

Tiaden, in his 'Das Gelehrte Ost-Friesland,' calls in question the authorship of this treatise, which he suggests was written by John the brother, not John the son, of David Fabricius. Not only, however, is his argument very weak that John the son was too young (he was born early in 1587, and would have been in his twenty-fifth year in 1611) to be likely to have made the observations in question, but, as Mädler remarks ('Geschichte der Himmelskunde,' vol. i. p. 207), many passages in the treatise are quite inconsistent with this theory. The author speaks of letters he had received from his father, David Fabricius, on the subject of the appearance of the Sun's disc, and afterwards of their observing the solar spots together after his own return from Holland. "Nolui (he writes) tamen mihi etiam in manifesto isto oculorum testimonio propter monstrosam et inusitatam Solis apparitionem assentiri. Vocavi ilicò parentem, cui tum temporis ex Bataviâ reversus aderam, ut ad hanc observationem, non quidem sine metuendo oculorum damno habendam, accurreret." In the preceding page we have the words "et parenti meo Davidi Fabricio." Undoubtedly David had a brother as well as a son named John; but these references are clearly to his son, who was therefore the author of this treatise. We do not know the Christian name of David's father; but we do know (from a memorandum of the son) that he died in 1608 at the age of 82.

David himself was cruelly murdered in his own garden in May 1617 by a peasant whom he had accused of theft. The date of the death of John Fabricius is not known, but it appears, from a letter of Kepler written in October 1616, that it took place more than a year before that of his father, so that he must have been less than thirty years of age at the time. The kind-hearted Kepler says in this letter (which is quoted in R. Wolf's 'Geschichte der Astronomie,' p. 317) "ich fühle dass du eines braven Sohnes, der die Philosophie eifrig pflegte, und ich meines Lieblings beraubt bin." His premature death was undoubtedly a loss to astronomy.

Blackheath, 1891, Feb. 4.

Yours faithfully,
W. T. Lynn.

Uncatalogued Comets.

Gentlemen,—

In your last Number (p. 98) the Rev. S. J. Johnson has called attention to three "uncatalogued comets." May I point out that they have all three been erroneously dated by Calvisius?

A.D. 535 should be 539, for which year Calvisius does not give any comet. Compare Burckhardt, Monatl. Corresp. xvi. pp. 499 *et seq.*, where the doubts suggested by Pingré are cleared up.

A.D. 599. This comet appeared in 595, and not in 599 (Pingré, i. p. 326).

A.D. 1505. Here, again, Calvisius has made a mistake; for 1505 read 1506. Lubienitzki (ii. p. 317) remarks that Calvisius in his Leipzig edition has given the correct year, 1506, but that he changed it to 1505 in the second edition.

The Observatory, Armagh,
1891, Feb. 11.

Yours faithfully,
J. L. E. Dreyer.

Lalande 11292.

Gentlemen,—

This star was rated 7th magnitude by Lalande, but I find with binocular that there is no star of even 8th magnitude near the position given in the reduced catalogue. About 1° due north, however, of Lalande's place I find a 6th-magnitude star, which is not given by Lalande or Harding. This seems to be B.A.C 1907 =D.M.+12°, 968, rated 6th magnitude by Argelander and Heis, and measured with the photometers at Harvard and Oxford. Lalande's place brought up to 1880·0 agrees fairly well with the position of B.A.C 1907, *if* we admit that there is an error of 1° in the N.P.D. in Lalande's Catalogue. It would therefore seem probable that for N.P.D 78° 13′ 27″·7 we should read 77° 13′ 27″·7.

Ballysodare, Ireland,
1891, Feb. 5.

Yours faithfully,
J. E. Gore.

OBSERVATORIES.

Leiden.—Two important volumes of Annals have recently been published, the 5th and 6th of the series. The 5th contains the second half of the zone observations between 30° and 35°, allotted to this Observatory in the programme of the 'Astronomische Gesellschaft.' There are about 10,000 observations, just as in Vol. IV., which similarly contains the first half; and as the zone contains something over 10,000 stars, each of which is to be observed twice, a few observations are still required to complete the work. After these are obtained we hope it will be found possible to print off the Catalogue speedily. The only noteworthy point in the reductions, beyond the great care with which they have obviously been carried out, is the curious correction to nadir-point depending on the time during which the observer has been reading the microscopes. Apparently his body gradually warms the instrument and the nadir slowly changes. The empirical formula deduced to represent the change is

$$a\left(-0{\cdot}5+\sqrt{2t+0{\cdot}25}\right),$$

where t is the time in hours and a a constant for the evening.

In the sixth volume the zenith-distances of fundamental stars observed from 1863 to 1874 are re-reduced, with special attention to systematic errors recently discovered and investigated. The present Observatory was founded in 1861, and the meridian-circle remained undisturbed till 1876, when it was sent to Messrs. Repsold for cleaning and repairing. The observations of fundamental stars made during this period were published in rather a scattered form, of which Dr. Bakhuyzen gives a comprehensive account; and the old reductions cannot be regarded as definitive. The present reduction was therefore undertaken, and Dr. Bakhuyzen has reason to be well content with his first victory over that terrible Old-Man-of-the-Sea, arrears of reduction. It is a little puzzling to find only ledgers of the results (in which, however, all daily results are deduced to 1865·0), and no definitive Catalogue. Perhaps this is reserved for a later volume.

Madras.—Mr. Pogson has produced the third volume of observations with the meridian circle, giving annual ledgers and catalogues of the stars observed in 1868, 1869, and 1870. Five more of these volumes are yet to come to complete the series of Separate Results from 1862 to 1887, some pressure having apparently been put on the Director to publish these results as soon as possible, rather than the observations of minor planets, variable stars, &c., to which he would have preferred to pay attention. The volume is in all essentials similar to the two preceding. (See 'Observatory,' Nos. 125 and 145.)

HARVARD COLLEGE.—Besides the Meridian Photometry noticed in our last number, we have received two other volumes of Annals dealing with Meteorology.

Also an interesting and well illustrated History of the Obsertory (32 pp., 8vo), during the first fifty years of its existence (1840–1890) by Mr. Daniel W. Baker, originally written for the public press, but containing matter of so much interest and value as to be worth preservation in a more permanent form.

WOLSINGHAM.—From Mr. Espin's report for 1890 we learn that "during the Spring the revision of the proofs of the last pages of the new edition of the 'Red Stars' was completed, and this work, which has been carried on since 1886, has been published by the Royal Irish Academy.

"The search for stars with remarkable spectra has been continued during the year. Some 70 have been detected, bringing up the total to 530. During this year only four stars of Type IV. have been detected; three of these are so faint as to leave some uncertainty as to their classification. The special study of certain variable stars has also been continued. The most interesting results are the establishment of the variation in the spectra of R Scuti and R Coronæ. The first varies from a scarcely perceptible third type to a normal third; the second from a continuous first type to a spectrum which resembles the fourth type—as would be anticipated! there is a marked variation in colour.

"Two new variable stars were discovered last year and announced by circular."

MELBOURNE.—Mr. Ellery's report for 1890 is more than usually interesting. He has succeeded in polishing one of the mirrors of the Great Equatorial, so that it is now of a more perfect figure than when it was first received at the Observatory; and the instrument is already quite fit for carrying on work in revision of the nebulæ, and for stellar spectroscopy. The other mirror is now on the machine.

Under date September 1890, the Astronomer reports that the building and dome are all ready for the photographic telescope, though no news had then been received of its shipment. He hopes it will reach them "before the summer has set in,"—a remark which puzzled us for quite five seconds, in spite of early teaching as to the habits of the Southern hemisphere.

With the transit-circle 3242 R.A.'s and 1782 N.P.D.'s were observed on the usual lines. The second Melbourne General Catalogue was published in 1890, February (see 'Observatory,' No. 160).

New quarters have been built for the Astronomer; and the Observatory has been lighted with electric light inside. Outside it is still quite dark, and dangerous even to the initiated; and the Board of Visitors point out that this is not nice.

Mr. Ellery notices in his report the necessity of reducing the number of Australian Meteorological Stations reporting by telegraph, as the telegraphic authorities may find the necessary work too burdensome. This is a fine opportunity for some one to make disparaging remarks about Meteorology.

WASHINGTON.—The U.S. Naval Observatory is apparently suffering from a dearth of workers. Naval Officers have recently not been spared in sufficient quantities, and consequently the work is behind. Thus the transit-circle work for 1885 is in print; the reductions for 1886 are nearly finished, and the apparent place reductions for 1887 are more than half completed: but those for 1888, 1889, and 1890 have not been commenced. Prof. Asaph Hall has been at work with the Equatorial on the satellites of Saturn and Mars, and on double stars, but here, again, computing help is wanted. Mr. Paul has chiefly worked at his own variable S Antliæ, whose period he finds to be $7^{\mathrm{h}}\ 46^{\mathrm{m}}\ 48^{\mathrm{s}}\cdot0$ within a fraction of a second, barely a third of that of the next shortest, U Ophiuchi. The report of Mr. Harkness rather takes away our breath, and we append it in full, for the benefit of those who like this sensation:—

"SIR,—I have the honour to report that during the year ending June 30, 1890, I had no assistants, and nearly all my time was devoted to investigations and computations connected with my work on the solar parallax and its related constants.

"As is well known, there exists a vast mass of astronomical, geodetic, gravitational, and tidal observations which have hitherto been discussed piece-meal, and have therefore yielded only isolated results. My object has been to deal with these observations as a whole, and to derive from them a homogeneous system of constants satisfying all the conditions imposed by our present theoretical knowledge. For that purpose recourse was had to the method of least squares, and a suitable system of simultaneous conditional equations was formed. The constants involved in these equations were the size of the Earth, the lengths of the seconds pendulum, the sidereal month and the sidereal year, the ratio of the mean motions of the Earth and Moon, the eccentricities of the orbits of the Earth and Moon, the inclination of the Moon's orbit, the rate of regression of its node, and certain co-efficients occurring in the lunar theory, and the first step was to ascertain the best values of these quantities. The observed quantities to be corrected by the least-square adjustment were the solar and lunar parallaxes, the constants of precession and nutation, the parallactic inequality of the Moon's motion, the lunar inequality of the Earth's motion, the constant of aberration, the light equation, the velocity of light, the flattening of the Earth, the masses of the Earth and Moon, and, indirectly, the masses of Mercury and Venus. In order to obtain proper values of these quantities for use in the conditional equations it was necessary to collect and discuss all the available astronomical, geodetic, gravitational, and tidal data; and, as this discussion included a special investigation of the masses of Mercury and Venus, the labour involved proved very great. Some unexpected delays have occurred, but the work is now so far advanced that 111 pages of it are in type, and it is believed that the entire paper will appear in the forthcoming Observatory volume for the year 1885.

"Very respectfully,
"Wm. Harkness,
"*Professor of Mathematics U. S. Navy,*
Of Executive Committee of Transit of Venus Commission."

"*The Superintendent of the Naval Observatory.*"

PUBLICATIONS.

Handbook of Practical Optics *.—The name of Steinheil is in itself almost a sufficient recommendation for such a book, and the practical optician who can read German will be very grateful to the joint authors. These were fired by a remark of Fraunhofer's, "Klügel's 'Dioptrik' is the only book a practical man can use," to attempt the production of a modern book on the same lines; and the verdict as to their success or failure must of course come from the practical optician. But as far as one who has not this qualification can judge, their prospects are good. The work is nothing if not practical. There are no attempts at elegant mathematical formulæ, which, indeed, we may as well be content to believe impossible in optics. The problems solved are of the most concrete form and are tackled straight forwardly and numerically. The study of a lens is conducted on the following plan: the courses of a central ray and a border-ray (Randstrahl), each supposed to consist of two definite colours, are traced through it and compared—numerically throughout. The appearance of such work is not pretty, but it is kept thoroughly neat and orderly, and one feels considerable confidence that the result is correct. In fact the book must represent a terrible amount of good honest grind, and we sincerely wish it every success.

NOTES.

The Spectroscopic Binaries.—Dr. Vogel has examined early photographs taken at Potsdam of the spectra of β Aurigæ and ζ Ursæ Majoris, whose binary character was discovered at Harvard from the doubling of the lines in their spectra: and in the case of β Aurigæ finds that the doubling is well-marked in many photographs from 1888, November 14, onwards, though no particular attention was paid to the fact at the time, and hence the discovery was missed.

* Handbuch der Angewandten Optik. Von Dr. Ad. Steinheil und Dr. E. Voit. Bd. I. Voraussetzung für die Berechnung optischer Systeme und Anwendung auf einfache und achromatische Linsen. Leipzig: B. G. Teubner Price 12 m.

These photographs amply confirm the results obtained at Harvard; they establish also a slight inequality of the components, which divides the whole period of 4 days into two dissimilar parts; that the spectra of the two components are similar from $\mu\mu$ 423 to 458; and that the maximum motion in line of sight reaches about 140 miles per second.

In the case of ζ Ursæ Majoris, however, the result of the examination is not so satisfactory, the lines being only double on one day out of 8. It is to be remarked, however, that the whole period is in this case 105 days, and the doubling can apparently only be seen for a few days at the proper time, which Prof. Pickering explains by a very elliptic orbit with major axis nearly perpendicular to line of sight. Still doubling might have been fairly expected on 4 of the Potsdam photographs, and yet in three of these cases nothing beyond a slight widening of the lines can be traced. (See Ast. Nach. No. 3017, p. 266.)

A NEW NEBULA NEAR MEROPE.—Mr. Barnard of the Lick Observatory, in communicating to the Ast. Nach. (No. 3018) some notes of his eye-observations of the nebula in the Pleiades, with the great Lick refractor, announces the discovery of a new and comparatively bright, round, cometary nebula, 9″ following and 36″ south of Merope. "It can readily be understood why this, the brightest of all the Pleiades nebulæ, has never been photographed. A sufficiently easy exposure to secure an impression of the nebula would so overexpose Merope that its light would coalesce with that of the nebula."

MASSES OF JUPITER AND SATURN.—M. Gustave Leveau finds from observations of Vesta the following values for the masses of Jupiter and Mars:—

Jupiter..........	1/1045·6
Mars	1/3700000

He cannot trace any secular effect of the action of the asteroids. We may remark that Dr. Haerdtl found a mass of 1/1047·18 ± 0·014 for Jupiter from observations of Winnecke's Comet (see 'Observatory,' No. 150, p. 254).

PHOTOGRAPHIC REFRACTION.—M. Prosper Henry calls attention * to the fact that the coefficient of refraction is not the same for photographic rays as for visual. In fact if the refraction be denoted by A tan Z.D., M. Henry finds that A is the following function of the wave-length λ:—

$$56''\cdot55 + 0''\cdot726\,\lambda^{-\frac{3}{2}}.$$

At Z.D. 80° the visual and photographic refractions would differ by about 5″; and generally in photographic work all visual refractions should be increased in the ratio 1·0156 : 1. The measures from which this result was obtained were made by

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* 'Comptes Rendus,' cxii. no. 7, p. 377.

placing a grating in front of the object-glass of a telescope and thus obtaining short spectra of stars.

The following opening sentences of a paper on "the Aryan question," by Prof. Huxley, seems to us sufficiently applicable in other cases to be worth reproduction here:—

"The rapid increase of natural knowledge, which is the chief characteristic of our age, is effected in various ways. The main army of science moves to the conquest of new worlds slowly and surely, nor ever cedes an inch of the territory gained. But the advance is covered and facilitated by the ceaseless activity of clouds of light troops provided with a weapon—always efficient, if not always an arm of precision—the scientific imagination. It is the business of these *enfants perdus* of science to make raids into the realm of ignorance wherever they see, or think they see, a chance; and cheerfully to accept defeat, or it may be annihilation, as the reward of error. Unfortunately, the public, which watches the progress of the campaign, too often mistakes a dashing incursion of the Uhlans for a forward movement of the main body; fondly imagining that the strategic movement to the rear, which occasionaly follows, indicates a battle lost by science. And it must be confessed that the error is too often justified by the effects of the irrepressible tendency which men of science share with all other sorts of men known to me, to be impatient of that most wholesome state of mind—suspended judgment: to assume the objective truth of speculations which, from the nature of the evidence in their favour, can have no claim to be more than working hypotheses."

LEXELL'S COMET.—Mr. S. C. Chandler contributes to the Astr. Journal (No. 231) a paper on the present aspect of the problems concerning Lexell's Comet. "Among the conclusions published a year ago, the writer indicated as the probable origin of the observed separation of this Comet in 1889, the action of the satellites upon it, in its passage through the Jovian System in 1886."

The recent discussion by Mr. Poor of the path of the comet allows of a more searching examination of this conjecture. It is conclusive that the comet almost certainly "passed the ascending node of its orbit upon that of the fourth satellite just inside the track of the latter; and if, as is abundantly consistent with the uncertainty of the calculation, a portion of the comet obtruded itself into the satellite's attraction-sphere, the result would be, with a low degree of cohesion, that this portion would be diverted for a time into a hyperbola around the satellite, and in emerging from its control, and in turn from that of Jupiter, would resume its course about the Sun as a separate body from the main comet."

MINOR PLANETS.—No. 303 was discovered by M. Charlois on Feb. 11; No. 304 by Prof. Millosevich on Feb. 12; No. 305 by Herr Palisa on Feb. 14; No. 306 by Charlois on Feb. 16. Nos. 290, 291, 292, and 295 have been named Bruna, Alice, Ludovica, and Theresia respectively.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 174. APRIL. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, March 13, 1891.

Lieut.-Gen. J. F. Tennant, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. Knobel and A. M. W. Downing, M.A.

The Minutes of the previous Ordinary Meeting were read and confirmed.

Mr. Downing. 109 presents have been received since the last Ordinary Meeting of the Society. Among them may be mentioned as calling for particular notice:—Prof. Newcomb, 'Discussion of Observations of the Transits of Venus,' 1761–1769,' and G. W. Hill, 'A new Theory of Jupiter and Saturn,' presented by the American Nautical Almanac Office; 'Catalog der Astronomischen Gesellschaft, Zone +65° bis +70°, beobachtet auf der Sternwarte Christiania,' presented by the Society; 'Catalog der Argelanderschen Zonen vom 15° bis 31° Südlicher Declination,' herausgegeben von E. Weiss, presented by the Vienna Observatory; Original Negatives of Jupiter, made with the 36-inch equatorial of the Lick Observatory, presented by Prof. E. S. Holden; R. Wolf, 'Handbuch der Astronomie,' Band II., presented by the Author.

A special vote of thanks was passed to Mr. E. Ristori for a present of an oxygen-bottle, and to Mr. W. Schooling for a stand for the lantern, for use at the evening meetings.

The President. Gentlemen, it has struck me that the number of admissions has of late been a good deal less than the number of elections to the Society, and I wish it to be well known that as a matter of fact, by the rules and bye-laws of the Society, a Fellow is not entitled to any of the privileges of membership until he is admitted either in person or by proxy. I mention this because it has been taken as a mere form without any validity, whereas, as a matter of fact, it is admission to the privileges of the Society.

Mr. Knobel read a paper by *Mr. A. A. Rambaut* "On the determination of Double-Star Orbits from Spectroscopic Observations of the velocity in the Line of Sight."

Since the beautiful results of Professor Pickering at Harvard on β Aurigæ and ζ Ursa Majoris, and Prof. Vogel at Potsdam on Algol and α Virginis, I have been examining the problem of determination of the orbit of a binary star from spectroscopic observations alone. The curve given in Prof. Pickering's report on the Henry Draper Memorial shows that with β Aurigæ there is a periodic displacement occurring in about four days. In drawing the curve of velocities from the data given there are two conditions that must be fulfilled. After giving these conditions Mr. Rambaut demonstrates his method of determination of the form of the orbit, and tests his elements with the data given for β Aurigæ, the errors being well within those of measurement.

One point which the examination of the question seems to indicate very clearly is that observations should be taken, not so much at equal intervals of time, but so that the points derived from them may be as far as possible equally thick along all parts of the curve of velocities, or, in other words, so that all parts of the curve may be determined with equal accuracy. It is important to fix the exact time of coincidence, as well as the epoch at which the velocity is the arithmetical mean between its two extreme values. With stars of the Algol type, where only one component is bright, displacement is measured by comparison with an artificial line; the whole velocity is composed of the orbital motion as well as absolute motion in line of sight. With sufficient and accurate observations it will be possible to separate orbital from absolute motion; but the materials at present available for Algol are almost ludicrously inadequate. The observations on which the curve of velocities for Algol depend are so few that the figures are little better than mere guesses.

A vote of thanks was accorded Mr. Rambaut for his paper.

Mr. Knobel read a paper by *Mr. S. W. Burnham* on "The Companions of Aldebaran."

The nearest companion discovered by Mr. Burnham with the 18½-inch Chicago refractor in 1877 is shown by later measures with the Lick 36-inch to have the same proper motion as the principal star, for both distance and position-angle are unchanged. The more distant Herschel companion has been observed for more than a hundred years, and its changes in position were ascribed solely to proper motion of the principal star; but Flammarion has shown that this will not wholly account for the changes, and that the fainter star has a proper motion of its own in a different direction. In 1888 the Herschel companion was found to be double with the 36-inch refractor, the distance between the stars being 2″. From the measures given it appears that the proper motion is common to both these stars; but more observations are required to settle this.

Mr. Knobel read another paper by *Mr. S. W. Burnham* on the Orbit of κ Pegasi.

In 1880 the larger star of this wide double was seen with the 18½-inch Chicago refractor to be a close double, distance 0″·27. Since that time observations have been made by Dr. Engelmann, of Leipsic, with a 7·5-inch Clark refractor, and these, with recent measures at the Lick Observatory, show that the motion of this star has been through 230° since its discovery. Mr. Burnham, from a consideration of all the measures of this rapidly moving binary, has been able to draw a satisfactory ellipse; and if this figure correctly represents the relative motions, this star has a shorter period than any known binary, its period being 11·3 years. The maximum separation is 0″·32 and the minimum 0″·08. The star should be comparatively easy to measure this year, as the distance will not be less than 0″·2. The wide double has been widening out and the position-angle slowly diminishing since 1831, and this is undoubtedly due to proper motion of the principal star.

The President. I shall be glad to hear if anyone has anything to say on Mr. Burnham's papers. The last paper seems to me a very important and interesting one. We have a star revolving in an orbit the plane of which is so inclined that the minor axis as viewed from the Earth is very small, so small that until quite recently it would have been almost unobservable. The major axis of the orbit is only about half a second in angular diameter, and yet Mr. Burnham seems to have got elliptic elements for this orbit. The star will be watched very carefully by other people, but I fancy very few will have the power of checking the orbit at all.

Mr. Stone. Did I understand that some of the observations gave the distance apart as one-tenth of a second of arc and gave position-angle with this separation?

Mr. Knobel. Yes! Mr. Burnham says the star will be comparatively easy to measure this year, as the distance will be not less than two-tenths of a second of arc.

The President. Mr. Burnham is speaking of a telescope such as we have not got in this country.

Mr. Stone. It is not a question of a very large telescope, but is one of being able to employ very high powers for these observations.

Mr. Downing read a paper by *Prof. H. G. von de Sande Bakhuyzen* (Foreign Associate), "On Variations of Latitude deduced from the Observations of Polaris made at Greenwich 1851–1889."

The accordance between the variations of latitude deduced from observations made in Berlin, Potsdam, and Prague in the years 1889 and 1890 is so evident, that it is impossible to explain them by accidental errors. Inequality of refraction at equal distances north and south from the zenith, produced either by anomalous refraction in the observing-room or by configuration of strata of uniform density in the atmosphere, which is not symmetrical north and south of the zenith, may account for some of these variations; but recent work by Prof. Albrecht and by myself has shown that this source of error cannot be admitted. By the courtesy of Prof. Helmert, I have received daily results for latitude from Berlin, Potsdam, and Prague from January 1889 to April 1890; and these show that the latitude from stars near the zenith is somewhat greater than that from all the stars, but the variation of latitude is about the same.

It is very improbable that the configuration of the strata of the air would have been exactly the same at these three places, and so have caused the same anomaly. In this state of the question it is very important to determine whether, in other observatories, the same variation of latitude has been observed, and whether the variations have a periodic character and the same values a certain number of years ago. The Greenwich observations from 1851 furnish the best material; and I have used those of the zenith-distance of Polaris for my purpose. The variations of this, as pointed out by Mr. Downing, by Mr. Thackeray, and myself, are not wholly the result of a real change of latitude, and the other causes must be investigated. Mr. Thackeray has investigated the errors of reading of thermometers owing to position, the method of adopting zenith-point correction, and the discordances of observations that may vary directly with temperature and with humidity; but the results of his inquiry gave no assurance that either correction was an unmixed advantage; while applied to low southern stars they are decidedly inadmissible. A correction depending on temperature will satisfy the discordances in the observations of Polaris. Prof. Bakhuyzen, continuing these inquiries, finds no indication of differences depending upon motion of the instantaneous axis of rotation, while discordances corrected for excess of temperature show differences greater than the accidental errors. The parallax of Polaris has a small influence, but its value is too uncertain to employ it in the reductions.

The conclusion arrived at, after careful discussion, is that the discordances in the zenith-distances of Polaris are, for the greater part, not caused by a real variation in latitude, but are chiefly an effect of temperature; that error of external thermometer will not explain discordances; that the differences between exterior and interior thermometers can explain the greater part of the discordances; that refraction in the observing-room is probably the cause; and that discordances corrected for that refraction are about the same for both culminations, and can be explained by real variation of latitude. Assuming the two latter probable causes, a discussion of the Greenwich observations shows that there is a maximum of latitude on February 9th and a minimum on August 9th, these epochs corresponding very nearly with those deduced from observations at Berlin, Potsdam, and Prague 1884–85 and 1889–60; but the amplitude of the variation is much smaller. There is a strong probability that the variations in these years were exceptional, and do not agree with the annual variations deduced from observations from Polaris at Greenwich during the period 1851–82.

The Astronomer Royal. I must express my feeling of indebtedness to Prof. Bakhuyzen for having taken up this subject, which is a very important and difficult one. I should not like to express an opinion upon such a technical paper without having an opportunity of considering it carefully. New screws in the microscopes may easily cause changes equal to the small quantities with which we are concerned in such a discussion. Then there is the all-important question of temperature in the observing-room. The transit-circle room at Greenwich is built with the gable running north and south, so there is an awkward angle somewhere near the position for observation of Polaris, and questions depending upon differences of reading of interior and exterior thermometers are raised. I am strongly of opinion that all these questions of astronomy of position depend very largely upon obtaining the true temperature of the air as affecting astronomical refraction, and it is an important point that the observing-room should be so arranged that you get strata of air which will be approximately circular in meridian section with the instrument in the centre. More consideration is wanted before coming to a definite conclusion as to whether we are dealing with a real quantity or something depending upon instrumental corrections, but at the same time I am thankful to M. Bakhuyzen for having taken up this question and given us his views upon the subject.

Mr. Turner. Perhaps, as some Fellows may not be quite familiar with this subject, I might say that Prof. Bakhuyzen has really been dealing with two questions. One question has been raised in Germany, as to whether the latitude is quite constant, and the Germans have been attacking that question by observing stars at equal zenith-distances on opposite sides of the zenith. They hope by this method to eliminate all questions of refraction. There remains then only a real variation of latitude. M. Bakhuyzen has been discussing the observations of Polaris at Greenwich with regard to this variation of latitude under the disadvantage that the observations are on one side of the zenith, and are therefore affected by refraction. It has been long known that there is some defect in refraction corrections, whether direct or indirect, with the result that the observations of the N.P.D. of Polaris do vary throughout the year; and this is the second question to be considered. Mr. Thackeray thought they varied with external temperature, but M. Bakhuyzen does not take this view. He says the differences between temperatures inside and outside the observing-room have most effect. The zenith-distances corrected for refraction have been examined to see if M. Bakhuyzen could detect any change in latitude to correspond with the change noticed in Germany, but no such change could be detected. In Berlin they found a change ranging over nearly half a second of arc in latitude, whereas for Greenwich I think M. Bakhuyzen found the change only something like one-tenth of a second of arc for the earlier years. On turning to the later years, from 1883 to 1889, he found at Greenwich that the change was in the same direction, but perhaps greater in extent.

Mr. Plummer. I think it would be interesting to know whether, in the opinions of the expert astronomers at Greenwich, there is any *real* evidence of a change in the latitude. The evidence I have seen does not seem conclusive to my mind, owing to its being mixed up with the question of the constant of aberration.

Mr. Turner. I believe the measures M. Bakhuyzen uses to deduce his results are first corrected to the new aberration.

Mr. Downing. But the correction is very small. Some years ago a proposal was made by some astronomer that to detect an annual period for this change of latitude, if it really exists, observations should be made at different parts of the year at observatories having the same latitude but of different longitude. I believe a programme of this kind was drawn up and a proposal made that three observatories in Europe and three in America should cooperate. That seems the most satisfactory way of arriving at a conclusion upon this matter, but I am sorry to say that so far as I know nothing has been done to carry out this much desired work.

Mr. Stone. I am afraid in questions of this kind there is a difficulty in arriving at any definite results. If you have your thermometers in the observatory a considerable distance from each other you cannot be sure you have them in positions that will give you the right refraction when you are dealing with such small quantities as those involved here. The real difficulty is the question of temperature, and there can be no doubt that this introduces uncertainties when you come to these small quantities, the uncertainties probably depending upon the exposure of the thermometer. I do not know of any method of deciding what is the proper exposure to give to a thermometer.

Mr. Turner. It is only fair to M. Bakhuyzen to say that he entered upon the discussion of the changes of latitude after a careful consideration and discussion of temperature, and after making every possible consideration for the thermometers exposed in different directions.

The President. I suppose there will always be a difference of opinion upon these small points until we find some perfect mode of providing for temperature, and there will probably always be some doubt as to which is the proper way of measuring that. In the meanwhile we may express our thanks to M. Bakhuyzen for the very interesting paper he has sent us.

The Astronomer Royal then read his paper entitled "Notes on the Preparations for the Work of the Astrophotographic Chart." I thought it might be interesting to Fellows to know, as we are now at Greenwich beginning operations for the Astrophotographic Chart, what the state of affairs is, and how far we have got, because it is a matter in which one must proceed tentatively, and there is a great deal to be worked out. With Mr. Turner's assistance this paper has been drawn up on the subject, and has been divided into different heads. The first important matter was to have a catalogue of guiding-stars, by means of which the telescope might be fixed with the centre of the plate in a definite position, so that we should have the centre of each plate in the selected position in the heavens, and thus be able to form a map. That is a matter which is not so simple as it appears at first. Prof. Bakhuyzen went into the question, and from his trials on a particular zone he came to the conclusion that if he went as far as 22′ from the centre, he would be able to get a sufficiently bright guiding-star within that distance in almost all cases; but we found there would be a great advantage in extending that distance from the centre. Mr. Hollis, who has had charge of this work, has tried the two plans, and finds there is a distinct advantage in taking 30′ as the limiting distance—that is to say, we are not obliged to take such faint stars. With the Greenwich 13-inch Equatorial there is a 10-inch guiding-telescope, and it is proposed to take, when possible, a star not fainter than 9th mag.; but in some cases it is necessary to go down to 9½, using, in such cases, the star of brighter magnitude which is nearest to the centre as an alternative guiding-star. In the paper which has been drawn up the numbers of guiding-stars are given of the different magnitudes, according as you take 22′ radius or 30′ radius. In the zone with which we are concerned there will be 90 stars of the 9½ mag. of Argelander (which really means anything down to 10th mag.) with 22′ as limiting radius, and there are only 28 of these stars with 30′ radius, so there is an obvious advantage in extending the distance from the centre. There is another point that has to be borne in mind, and which is obvious as soon as we begin to think of it, and that is that in order to get the position of the centre of the plate accurately, we must allow for the effect of the projection of the sphere on the tangent plane of the plate in taking the rectangular co-ordinates of the guiding-stars. The next is the réseau which is to be printed on the plate. Two very fine réseaux on glass have been received from Prof. Vogel, and one has been mounted. This work has proceeded slowly owing to the necessity of testing things after each step, and each particular step has taken some time to properly arrange. The réseau consists of two sets of lines, ruled at right angles to each other on the silvered surface of a glass plate, the lines being 5 millimetres apart. The lines are cut through the silver on the glass plate; and the sensitive plate is put nearly in contact with the réseau and placed in front of the object-glass of the photographic telescope. By means of an incandescent electric lamp placed in the focus of the object-glass the réseau is printed on the sensitive plate, but is not developed until the plate has been exposed on the stars, the stars and the lines of the réseau being developed at the same time. The réseau thus serves to give reference lines for the stars, and also for the determination of any distortion of the film. There can be no doubt, from our experiments, that this method is perfectly successful, very beautiful impressions being thus obtained, and our best thanks are due to Prof. Vogel for having worked out this valuable adjunct to the photography of the heavens. We have also discussed the determination of errors of the réseau, because we did not like to assume that it had been perfectly ruled. The results so far show that the errors of ruling are very small, that the spaces between the lines are sensibly correct, and that equality obtains throughout the whole length of the lines. The lines are parallel curves, and that is an important point to determine, because Prof. Vogel in giving the errors of the réseau only gave them for one part of each of the lines and assumed that they held throughout. There is another interesting point, and that is the relation of the weather to the course of the work. This year the weather has been not at all favourable, and we have been much hampered by clouds and the severe winter. In February, although we had many fine nights, there was much fog, so I am not sure that this year can be taken as a fair sample of the amount of work we shall get done in a given time. I hope it is not. Besides weather, we have to consider that the work is only just commencing, and we have not yet got into the way of it. Much time has been spent in instructing many members of the staff, so that the results are not comparable or commensurate with the work done or the time given to it. From Feb. 9 to March 12 there were 29 nights continuous watch: 15 nights were cloudy, 3 were partially cloudy, and 11 were available in some degree for photography, although several of these were more or less foggy. On these nights 21 photographs were taken for star-trails or regular work representing an exposure of 12 hours 49 min. Better results will we hope be obtained as the work becomes more familiar. It is important to bear these difficulties with weather in mind, because some gentlemen who have looked at the work from a theoretical point of view have suggested going beyond the 14th mag. But this would mean increased exposure, and would very materially add to the difficulties of getting results, and would lead to much delay in the completion of the work.

One of the resolutions of the Committee was that each Observatory was to select the best plates that could be obtained. No decision was arrived at as to what maker was to be selected to supply the plates, and accordingly, at Greenwich, Paget, Star, Cramer, Seed, and Mawson-Swan plates have been tried. Our thanks are due to the various makers for the trouble they have taken in making plates and in cutting them to the proper size. After careful trial a slight preference is felt for the Star plates.

Another question dealt with has been that of exposure. A resolution of the Committee instructed each observatory to practically ascertain the correct exposure to give for stars down to the 11th magnitude, and plates for this purpose have been exposed, in the region assigned to Greenwich, with exposures of 30 min., 6 min., and 1½ min. respectively. Mr. Turner has examined these plates, and finds that all the stars in Argelander are shown with 1½ min. exposure, that is all down to practically the 10th magnitude. He has also measured the diameters of the star-images. Fellows may be aware that there has been some question as to whether the diameter of a star-disk on the film varies as the square root of the time of exposure, as the cube root, or as the fourth root. Mr. Turner finds that on comparing the plates with 30 min. exposure with those of 6 min., the diameter varies between the cube root and the fourth root. A value of 1/3·3 was adopted as the index, and with that what may be called the constant of the plate was determined, and a method devised for calculating the time of exposure required to give certain diameters to stars of certain magnitudes. Taking the smallest diameter of the disk as 2″, 12 min. exposure are required for a 14th mag., 0·8 min. for an 11th mag., and 7 sec. for a 9th mag. star. These results are quite preliminary, but they represent a fairly average night, and give some interesting information.

Disks.	Magnitudes.		
	14th.	11th.	9th.
″	min.	min.	secs.
5	252	16·0	150
4	120	7·6	72
3	46	3·1	28
2·5	25	1·6	15
2	12	0·8	7

These figures are to some extent provisional, the work having been undertaken to settle the preliminary questions before the actual work of the star-charting can be systemically undertaken; and not to give any scientific determination of accurate values. I do not know whether Fellows are aware of the precise conditions under which the preparation of the star-map is to be carried out. The Paris Conference decided on two sets of plates—one of stars to the 14th mag. for the star-maps; and one with two exposures, one exposure to give stars down to the 11th mag., and the other, after a slight displacement of the plate, to give stars of 9½ mag. From the Greenwich preliminary work Mr. Turner has found that exposures of 25 or 30 minutes will be necessary to get to the 14th mag., and for the other plates (the catalogue plates) 2 min. and 30 secs. should be given. Those are our chief results. Considerable time was spent in alterations of the instrument; and the bad winter came upon us before we could get properly to work, or the results would have been much fuller.

Mr. Stone. Have you made up your mind as to the minimum size of the disks yet?

The Astronomer Royal. No, that is a question for determination.

Mr. E. Robinson. Will it not be difficult to get uniformity of disks on different plates?

The Astronomer Royal. The object is to get stars down to a certain magnitude. If you can see the images, that is all that is absolutely necessary.

Mr. E. Robinson. There is no agreed standard?

The Astronomer Royal. No, it is a matter for experiment.

Mr. Plummer. As representing one of the Observatories engaged in taking one of these zones, I have to thank the Astronomer Royal for the information he has put before us. Those sharing in the work will find great advantage from comparing results, and since some of my results, obtained at Oxford, do not agree with those obtained at Greenwich, a discussion on the points raised will be of great interest. On the question of the distance of the guiding-star from the centre of the plate, I think the determination to increase the distance is very wise. After trial with faint and bright stars for guiding-purposes, I find the distortion which comes into the guiding-telescope with increase of distance is of less consequence than the employment of a faint star; but, as the Astronomer Royal has mentioned, the increased distance introduces the mechanical difficulty in settling the exact point in the centre of the plate, and I find I cannot get the co-ordinates exact to the 5″ limit assigned by the Conference, when working outside the 22′ field, although I hope to overcome that difficulty. I have nothing but praise for the accuracy and scientific usefulness of the réseau. At Oxford, to print it on the plate, we place a small incandescent lamp in the focus of a reflector, and use the parallel rays. Dr. Gill when at Oxford, while examining our arrangements, kindly undertook an examination of the uniformity of the lines on our réseau, and if others who have time will undertake similar work much saving of time to the workers will be effected. The errors of our réseau are, I believe, very small indeed. On the point of exposure I cannot agree with the Astronomer Royal. The Pleiades have engaged our attention during the winter, and we have photographed them with certain exposures, and increased or diminished the time by a known ratio on the same plate. On measuring the images I found 17 to 20 secs. necessary to give a measurable disk of 3″ diameter to a 9th magnitude star on fine nights, longer exposure being necessary on other nights.

The Astronomer Royal. What is the zenith-distance?

Mr. Plummer. The declination is 22° N., therefore 30° is the least zenith-distance.

The Astronomer Royal. The greater zenith-distance would probably make the difference.

Mr. Plummer. With our telescope a star-disk of 2″ is not discernible unless you know where the star is on the plate.

The Astronomer Royal. A disk of 2″ diameter would be given for a 14th mag. star with 12 minutes exposure according to our results, but in our notes we say 25 to 30 minutes will probably be required to give satisfactory disks.

Mr. Plummer. The conclusion I have come to is that 90 minutes are necessary for a 14th mag. star on an average night.

The Astronomer Royal. That exposure is impracticable.

Mr. Ranyard. What plates have you been using?

Mr. Plummer. The same as the Astronomer Royal. The illness of Prof. Pritchard, in which we all sympathize with him [hear, hear], has prevented me from putting our notes fully before the Society.

Mr. Knobel. Can you give us any idea of the probable error of gradation of the réseau? that would be interesting.

The Astronomer Royal. We had a micrometer, used for measuring the Transit of Venus photographs, altered for this work, so that our measurements must not be considered as absolute or final. The millimetre scale of the instrument is not absolutely accurate, but, as far as the errors of this are known, we find the réseau errors are too small for us to detect with absolute certainty; they are about the same as the probable errors of the determination.

Mr. Plummer. Do the Greenwich authorities find that the same magnitude is impressed in the same time all over the plate? I found a decided want of smaller stars as I passed from the centre of the plate, and this at 50′ or 60′ became more emphasized and accentuated.

The Astronomer Royal. This would entirely depend upon the position of the plane of the plate with regard to the focal plane of the telescope. At Greenwich and at Paris the plate is put well within the focus for centre of field so as to equalize the error from curvature of the field. With this precaution you will get satisfactory results for a region 50′ or 60′ from the centre of the field. I should like to know the exact position of the plate with regard to the focal plane at Oxford, as if it is inside the focal plane I do not think the objection raised by Mr. Plummer is valid. On several plates taken at Greenwich, which were specially compared with Argelander's charts, the images of the fainter stars in Argelander were distinctly visible up to the edge of the plate.

Mr. Ranyard. Plates differ with different instruments. I have been examining plates taken by Mr. Russell at Sydney, and in those the falling-off in number is exaggerated owing to the larger field. This falling-off must depend upon the correction of the photographic object-glasses.

The Astronomer Royal. I was speaking of what we may call a perfect object-glass for photographic purposes—that is, one that will give a lateral image that is symmetrical, not only with reference to the radial plane, but to the transverse plane.

The President. Has the Astronomer Royal heard of an object-glass by Steinheil, who claims that it gives circular images?

The Astronomer Royal. I did not understand him to say circular.

The President. He said he had made an object-glass with which he was going to get circular images in all parts of the field.

The Astronomer Royal. What he got was a symmetrical image. I think it is geometrically impossible to get a circular image all over the field.

Mr. Turner. I firmly believe that Mr. Plummer and I have arrived at the same facts, but expressed them in a different way. I believe Mr. Plummer really agrees with that table. It does not represent anything theoretical; it is simply experimental. I do not mean to say I have measured a disk of 2″. The table is only extended to 2″ by carrying on the formula. 2″ is the smallest disk you can be certain of. You get a well-defined disk of 3″, but with 2″ you can sometimes only see a star when you know where it is. Are you to consider as the standard the exposure with which you get a perfectly unmistakable star, or a star that you can find only when you know where it is? The 14th mag. is unmistakable with 30 minutes exposure.

Mr. Plummer. The 14th mag. are to be left in the chart-form and not measured. If you are going to print you lose at once a very considerable number of stars.

The Astronomer Royal. It is not proposed to print the 14th mag., I believe.

Mr. Isaac Roberts. The drawings that the Astronomer Royal has made of star-images apply to the reflector as much as to the refractor. It is therefore a mechanical difficulty, and not simply due to the marginal rays not coming to the focus. The Companion to Polaris is about the 9th mag. I have been able to photograph it on some nights in a single second, and at other times it has taken one minute, so great is the variation in exposures due to atmospheric causes.

Mr. Knobel. The difference of conclusion between Mr. Plummer and the Astronomer Royal with regard to the time of exposure for taking 14th-mag. stars is remarkable, and I would impress upon observers the very great importance of getting plates not only from the same maker but of the same making, and made on the same day, because plates made on one day differ from those made on another. I am informed by a large photographic plate maker that plates which are made on different dates are not comparable.

Mr. Ranyard. I think they should not only be made on the same day but at the same cooking.

The Astronomer Royal. During the process of taking photographs we ought to take trial photographs on the same plates of a star like Polaris, and I am making arrangements to have a 4-inch Dallmeyer object-glass mounted at Greenwich so as to take test photographs every night. Then we can deduce the exposure required for the plate for the map and be independent of the makers. The changes in the atmosphere are far greater than any change we have to anticipate in the plates.

Mr. Ranyard. I think Mr. Roberts will corroborate me that the changes in the night are so great that there is a change of a whole mag.

Mr. Roberts. That is quite true in the case of Sappho. In some cases the trail is well defined in parts, while in other parts there are great differences in width of the trail; and all within one hour.

The President. Our cordial thanks are due to the Astronomer Royal for his paper, and for the great amount of information he has caused to be given to us by others. There is no doubt that the plates vary to some extent, but I think we are only beginning to find out the difficulties of making a photographic chart.

Mr. Downing. Mr. Marth has sent a note, in the course of which he says:—The planet Venus will pass along Jupiter's system on February 6th, 1892. There is, however, a reason why the subject should be mentioned at the March meeting. At the time of the conjunction of the two planets they will be 33°·5 east of the Sun. Jupiter reaches the corresponding western elongation on March 29 (Easter-day), and it will be well to call the attention of observers to the fact, so that before the end of the present month they may ascertain, by midday observations of Jupiter's system, whether their telescopes are powerful enough for observing the conjunction on February 6th, 1892, which in our terrestrial longitude occurs in the forenoon.

The following papers were announced:—

A. A. Rambaut. "On the Determination of Double-Star Orbits from Spectroscopic Observations of the Velocity in the Line of Sight."

S. W. Burnham. "The Companions of Aldebaran."

J. Kleiber. "Catalogue of 918 Orbits of Meteor-Streams, from the observations of Mr. W. F. Denning."

Cambridge Observatory. "Mean places of Comparison Stars for the Planets Victoria and Sappho, observed with the Transit-circle."

J. Gledhill. "Observations of the Satellites of Saturn: Conjunctions with the Centre of the Planet, observed at Mr. E. Crossley's Observatory."

J. E. Gore. "Observations of the Variable Star S (10) Sagittæ."

S. W. Burnham. "The Orbit of κ Pegasi (β 989)."

Sydney Observatory. "Measures of Double Stars made in the years 1882–1889."

A. M. W. Downing. "Comparison of the Star-places of the Greenwich Ten-year Catalogue with those of the Second Melbourne General Catalogue, and with those of the Cape Catalogue for 1880."

John Tebbutt. "Double-Star measures made at Windsor, New South Wales, 1889–1890."

A. M. W. Downing. "Note on the Orbit of Juno."

Astronomer-Royal. "Notes on the Preparations for the Work of the Astrophotographic Chart."

H. G. van de Sande Bakhuyzen. "Variations of Latitude deduced from the Observations of Polaris made at Greenwich, 1851–1889."

Radcliffe Observatory, Oxford. "Observations of the Planet Victoria and comparison stars, made with the Transit Circle during the opposition of 1889." (Communicated by Mr. Stone).

A. Marth. "Ephemeris for Physical Observations of Jupiter, 1891."

Thomas Richard Clapham, Austwick Hall, near Settle, Yorkshire; *Arthur Kennedy*, 1 Clement's Inn, W.C., were duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Alfred Henry Fison, D.Sc., University College, London (proposed by F. W. Levander); *Thomas Gordon, F.R.Met.Soc.*, 9 Scotch Street, Whitehaven (proposed by Sir R. S. Ball); *George E. Hale, B.Sc*, 4545 Drexel Boulevard, Chicago, U.S.A. (proposed by S. W. Burnham).

BRITISH ASTRONOMICAL ASSOCIATION.

The fifth ordinary Meeting of this Association was held in Barnard's Inn Hall, Holborn, on March 25, 1891, the President, Captain W. Noble, in the Chair.

The names of nine candidates for admission were read and ordered for suspension, and eighteen new Members were elected.

Mr. Elger, the Director of the Lunar Section, read a paper on Ptolemaus, which is one of the formations especially under study by the Section at the present time, illustrating his remarks by a series of slides, showing Ptolemaus under high and low illumination. Mr. Elger drew especial attention to the true scale of the formation, which covers an area equal to that of Lancashire, Yorkshire, and Westmoreland, pointing out that what observers are accustomed to speak of as pits, holes, mounds, or hillocks, are really very large objects, that many of the minor features on the floor of Ptolemaus are really as large, not merely as the crater of Vesuvius, but as the entire mountain. He strongly recommended draftsmen not to attempt too much at one sitting. It was far better to delineate thoroughly a small portion of the surface than to make a hasty and imperfect sketch of a large district. He mentioned also that he had discovered a new straight ray on the floor, and specified several other points upon which the work of the Section had increased their knowledge of the details of the formation.

Mr. Green gave an address on the Lunar Seas, also illustrated by lantern-slides. He said that in his paper he was rather going to take an opposite course to that recommended by Mr. Elger, and deal with general views rather than with minute details. It was argued by Nasmyth and Carpenter that the Lunar Seas were the oldest formations on the Moon. He thought he could show that was not the case. First of all he would point out that the largest seas were near the centre, and that they decreased in size towards the limb, and there was no trace of a sea on the limb itself at any period of the libration. That looked as if position relative to the Earth had something to do with them. Then we found craters with their seaward walls broken down round the margin of the seas, *e. g.* Fracastorius on the Mare Nectaris; the Sinus Iridium on the Mare Imbrium, and so on. The relics of the missing wall looked like a piece of melted material. On this view Pico and other isolated mountains were to be looked upon as the remnants of ranges. The Appenines appeared as if they had been undermined by some liquid, for great masses which appeared to have fallen from their summits lay parallel to them. Mr. Green then explained how he thought that the lunar tides might have given rise to the formation of these seas.

Other papers announced were:—"The Canals of Mars," by A. Stanley Williams. Three papers on the "Determination of Star Colours," by W. S. Franks, the Rev. W. R. Waugh, and B. J. Hopkins respectively. "Selenographic Studies," by Victor Neilsen. "Is the Moon cased in Snow?" by S. E. Peal. "Sun-glows," by the Rev. S. J. Johnson. "Two New Red Stars, and a possibly Variable Star," by the Rev. T. E. Espin. "John Michell," by J. R. Sutton. "The companion of Sirius," by J. E. Gore. "Sketches of Jupiter and of the Zodiacal Light," by E. M. Antoniadi.

ROYAL METEOROLOGICAL SOCIETY.

The twelfth annual Exhibition of Instruments was opened on March 3, in the rooms of the Institution of Civil Engineers, 25 Great George Street, Westminster. The Exhibition this year was devoted to Rain- and Evaporation-Gauges, and new instruments constructed since the last Exhibition.

Almost every known pattern of rain-gauge that has been used in this country was shown; most of the gauges have funnels 5 or 8 inches in diameter. The Meteorological Office 8-inch gauge is generally regarded as the best gauge for ordinary observers to whom cost is not a primary object, as it has all the good features of the Glaisher and of the Snowdon patterns, and, being of copper, is very durable. In mountainous districts, where the rainfall is heavy, and the gauges can only be periodically examined, gauges capable of holding 40 or 50 inches of rain must be used. Specimens of these gauges, as well as of the rain- and snow-gauges used in France, Germany, Russia, Switzerland, and the United States, were shown, as also were some interesting storm-gauges and self-recording gauges. The evaporation-gauges included several instruments employed for measuring the evaporation from a free surface of water, and others for use with growing plants. A number of new instruments, among which are various anemometers, recording barometers, and cameras for meteorological photography, and an interesting collection of maps of rainfall over the British Isles and various parts of the world, as well as numerous photographs of floods, meteorological phenomena, &c., were also on view.

The usual monthly meeting was held on March 18, Dr. C. T. Williams, *Vice-President*, in the Chair.

Mr. G. J. Symons, F.R.S., read a paper on the 'History of Rain-Gauges,' from which it appears that Sir Christopher Wren, in 1663, designed not only the first rain-gauge, but also the first recording-gauge, although the instrument was not constructed till 1670. The earliest known records of rainfall were made at the following places:—Paris, 1668; Townley in Lancashire, 1677; Zurich, 1708; and Londonderry, 1711. Mr. Symons gave a very full account of the various patterns of rain-gauges, and in most instances pointed out the merits or defects of each.

Mr. A. W. Clayden, M.A., showed, on the screen, a number of interesting transparencies of photographs of clouds, lightning-flashes, and other meteorological phenomena.

Notes on Visits to some American Observatories.

Last autumn (October, 1890) opportunity offered itself for visiting America. The dismounting of the 25-inch refractor at Gateshead was begun in September, and the work was well advanced by October 15th, for I had seen the telescope dismounted without hitch or accident, and had realized afresh to what a colossal creature the disjecta membra belonged; had escorted the object-glass to Cambridge, and safely deposited it in the library of the Observatory, where it received a warm welcome; had seen the preliminary trenches dug for the foundations of the building on the new site; and had left Messrs. Wailes & Co.'s workmen at work on the dismantling of the iron dome and outbuilding at Gateshead. The work was then well in hand, and Mr. Common's kind consent to have any questions that might arise referred to him, relieved my mind of all need for anxiety, though it was accompanied, like other brain-tonics, with a slight bitter taste in the shape of a suggestion that on my return he would welcome me in the capacity of Hungry Editor. Many a time, in hearing of some special bit of work accomplished, I have thought "this will be news to interest readers of 'The Observatory';" but on my return, I glance through the numbers of 'The Observatory,' and find with dismay how terribly up to date the Editors have kept the public. All my interesting points are stale news, and it seems as if all I can do will be to hold a kind of audit, and add signature to a statement to the effect that the items of news from certain American sources have been examined and found correct. I cannot hope to impart the vividness that comes of being able to connect the personality of the worker with the account of his work, but the instructions for people who wish to attain it are simple enough—"'go and do likewise." The carrying out of these instructions may be another matter; but if any one is induced to make the trial, he will find (if I may generalize from my own experience) the kindest possible welcome and open doors to mines of interest in results and methods.

A pilgrimage to Mount Hamilton does undoubtedly involve a great deal of travelling; but when the route chosen carries one into Canada up that magnificent front drive—the St. Lawrence—through that magnificent front gate—Quebec—to Montreal; over the Canadian Pacific Railway to Vancouver and Victoria; on a peaceful Pacific round Cape Flattery, and through the Golden Gate to San Francisco, there is no thought of hard peas beneath one's feet. So from our start from Liverpool, October 23rd, to our arrival in San Francisco on November 23rd, the time seemed short when we considered that in that month we had spent five days in Montreal, and a total of five or six more in the Rocky Mountains, Vancouver, and Victoria.

At the Lick Observatory, Prof. Holden gave us—my wife accompanied me in these journeyings—a very hearty welcome, and we spent three splendid days and nights in becoming initiated into the mysteries and beauties of the top of Mount Hamilton. From San Francisco a journey of two hours in a train took us southwards through the wide and extraordinarily fertile Santa Clara Valley to San José, and thence a pretty pair of chestnuts dragged us up the hill road 27 miles. The big dome of the Lick Observatory is visible from San José as a white spot on the hill top, 4209 feet above the sea. Though so late in autumn the weather was wonderful; there had, however, been no rain in September, and so we learnt something of the difficulties of life up there. Our host, in greeting his dust-bestained guests, said that "we might *drink* water, and now and then wash our faces, but baths were out of the question." Now the drive had been very dusty, and I fear we washed our faces too often, for on the last night of our visit there was not enough water to work the engines to turn the dome of the big refractor. Three days later the long-expected rain came, and we felt glad that the siege of the astronomers had been raised, but still more glad (I may confess) that we had visited them during the siege, and had not come in for the downpour.

The sight of the sky at night was superb. In the daytime I was surprised to see how strongly polarized the light from the sky was, judging at least from the intensity of "Haidinger's brushes;" this intensity may, however, have been due to increased sensitiveness of the eye in unusual surroundings. There was undoubtedly a large quantity of dust in the air; and at sunset there rose from the plains the blanket of fog to which is ascribed the steadiness of the atmosphere above the mountain; only the highest peaks rise above the fog. It was inevitable that our visit should fall on nights near full moon, but even with the disadvantages caused by moonshine in observing, it was worth foregoing some of the observing to realize what moonshine can be.

That evening we spent some time in the big dome, and Mr. Burnham showed some extraordinarily fine doubles. The fineness of the star-images and the steadiness of the atmosphere made one realize what magnificent opportunities are there. The companion which Burnham has found to Herschel's companion to Aldebaran was splendidly clear. [This I failed to see at Washington on a good seeing night with the 26-inch, but at Princeton Prof. Young and I saw it with the 23-inch.] A very fine double close to Alcyone with separation of about 0″·25 was well seen. It was striking to see the ease with which the instrument was handled. Mr. Burnham's son assisted him. The dome was turned easily and quietly by means of a water-engine. A very convenient homemade chair of Mr. Burnham's device was in use on the movable floor. The movement of the floor seemed to be effected without difficulty, as I saw next morning; but alterations are contemplated which will make it more advantageous and convenient.

Later in the evening, Prof. Holden showed me numbers of photographs taken with the big refractor, some of the most interesting being short-exposed plates of nebulæ, in which the nucleus of the nebula makes its mark as a fine point hardly distinguishable from neighbouring 8th or 9th magnitude stars. These are taken with a view to deciding the question of the existence and magnitude of the parallax of nebulæ. Special photographs have to be taken to determine the exposure suitable for each nebula. Prof. Holden had just devised arrangements for taking enlarged photographs of details of the Moon, the clockwork being in this case a source of difficulty. Messrs. Barnard and Burnham are both finished photographers, and some of Mr. Burnham's photographs bearing upon the question of short exposures for showing minute differences of intensity were extremely interesting.

Mr. Barnard was most kind in showing on our third night at Mount Hamilton the admirable qualities of the 12-inch. This was originally the property of Dr. Henry Draper, and was only given up by him when he got the 11-inch photographic glass which is doing such good work at Harvard in Prof. Pickering's hands. Mr. Barnard was still perturbed by his observations on the abnormal appearance of a satellite of Jupiter, which was seen to be either double or belted, or in some way distorted whilst passing in front of Jupiter's disc. But these mental perturbations were rather hidden by the satisfaction in recovering a comet for some time lost to sight. This comet is 1889 *d*, discovered by Brooks in July 1889, and is probably destined to become notable, inasmuch as Mr. Chandler, of Cambridge, Mass., welcomes it as Lexell's Comet, and shows that it has passed so close to Jupiter, that a satellite must have passed through its tail. Mr. Barnard got observations of it in November, and has, I hear, got later ones in December; these will be of very great interest. It is to be noted that this is the recovery of a comet too faint for smaller instruments.

It was a disappointment to me not to meet Mr. Keeler, who has charge of the spectroscopic work with the 36-inch. He was absent from Mount Hamilton at the time. He had lately got very satisfactory results in measurement of motion in the line of sight working with a diffraction-grating in spectra of the 3rd and 4th orders. The mode of checking results by measurement of motions otherwise known, *e. g.*, the motion of Venus, has given great weight to measurements of motions not otherwise known. We must join Mr. Maunder in congratulating Mr. Keeler on the excellent agreement of independent measurements of such small velocities (' Observatory,' No. 168, p. 354).

The very difficulties met with in using a large refractor like the Lick telescope form a subject of extreme interest. One has food for reflection when one realizes (i.) that the position of the photographic plate may vary as much as two inches along the axis of the telescope to give the best results in enlargements according to the kind of object photographed and according to the kind of detail desired; (ii) that the collimator tube of the spectroscope has possibility of motion 4 inches along the axis in order to give the best results in different parts of the spectrum; and (iii) that a keen observer (Mr. Keeler) is able to detect stars of the Wolf-Rayet type in virtue of the dispersive power of the objective alone, which objective has been carefully and satisfactorily corrected for achromatism, and compares very favourably with, for instance, the Vienna and Pulkowa glasses.

For the satisfactory utilization of the 36-inch in various lines of research, Prof. Holden has arranged that for two nights a week photographic work goes on under his direct care, for two nights a week Mr. Burnham uses it for micrometric work, and for two nights a week Mr. Keeler is doing spectroscopic work. Mr. Schaeberle has charge of the meridian work. It may be a question how far it is desirable to employ one telescope in so many lines of research; but the question presses perhaps less for solution on Mount Hamilton than in other places, on account of the very large number of observing nights; this makes it desirable to double the staff working with the one instrument, and practically duplicates the instrument from the standpoint of less favourably situated observatories. So each observer works on his two nights as if all the other nights were cloudy, and without doubt these other nights are bad observing nights for *him*. And one cannot but be struck with the spirit that prevails, and which makes one feel that the Lick Observatory is essentially one of the world's observatories, apart from any nationality.

After leaving Mount Hamilton, we spent three days at Monterey, a most beautiful spot on the Pacific coast, and thence went to San Francisco, to begin our homeward route. For the sake of southern climate we chose the Southern Pacific or Sunset Route, and so passed through California to Los Angeles, and across Arizona and Texas to New Orleans; and in a fortnight's time, which included a digression to see old friends in the heart of Florida, and also a snowstorm in Virginia—the so-called "severest for 50 years"—we reached Washington in the last days of December, and felt that we were in a land more densely populated with observatories.

Cambridge, 1891, Mar. H. F. NEWALL.

[To be continued.]

A Visit to the Observatory at Windsor, N.S.W.

THE morning in January on which I visited Mr. Tebbutt's observatory at Windsor was not a typical day of sunny New South Wales, but reminded one more of Old England, it being dull and overcast and by no means warm.

Windsor is about 30 miles distant from Sydney in a north-westerly direction and I proceeded to it by train; Sydney is soon left behind and we run through pleasant suburbs, here and there having glimpses of pretty villas with their broad verandahs, half hidden behind the profusion of a semitropical vegetation. These soon become scarce and we begin to traverse tracts of bush, as the Australian woods are called, occasionally cleared to make room for orange-groves and vineyards. We speed past Parramatta (well known to astronomers by Sir Thomas Brisbane's catalogue of stars), and in due course reach Windsor, an old town for Australia. Strangers need have no fear of not finding their way, for everybody knows Mr. Tebbutt, whose name, indeed, has become a household word in Australia through his endeavours to create an interest in our science.

The observatory is situated on rising ground a little way back from the road and not far from the junction of a small stream with the Hawkesbury river. I mention this, as, although the observatory is 50 feet above the river-level, it has been flooded. In 1867 the river rose no less than 62·7 feet, flooding the observatory and also Mr. Tebbutt's private dwelling to the ceilings on the ground-floor. Floods of 40 feet are not too scarce but they do not invade the buildings. A splendid horizon is formed on the north-west by that strange range, the "Blue Mountains," and such is the extreme purity of the air that Mr. Tebbutt tells me he can distinctly see stars set behind them.

The observatory has been well described by Mr. Tebbutt in his 'History and Description' of it, published in 1887, and I need only remind readers that the chief instruments are an 8-inch equatorial refractor by Grubb and another of 4½ inches by Cooke, and a transit instrument of 3 inches clear aperture very solidly mounted after Mr. Tebbutt's own designs. To be sure of the workmanship, Mr. Tebbutt built the masonry for his transit instrument *himself*, and, in reference to the many mechanical refinements upon refinements seen in some observatories, he remarked: "Ah! I don't believe in too many; those telescopes never do much work."

In a country where astronomy is but little thought of, it is pleasing to come across such a bright exception as Mr. Tebbutt, himself an Australian who has never been out of New South Wales, and who truly loves astronomy for its own sake, and who has pursued it with unflagging devotion for now nigh on 30 years, and, as he modestly remarks, "doubtless with some service to the cause of Astronomy."

The long list of contributions to the pages of the 'Observatory,' 'Monthly Notices,' and 'Astronomische Nachrichten' testify to this latter fact; indeed, situated as Mr. Tebbutt is, in the Southern Hemisphere, his long series of observations is invaluable. We may remark, too, that not only has he equipped and kept up the observatory at his own expense, but he has also made all the observations and himself computed nearly all the reductions; and this, for an unbroken period of over a quarter of a century, and so far distant from those taking an interest in his pursuits that it creates, as he plaintively remarked, a feeling of isolation.

I saw the observation journals and forms of reduction, all most methodically kept and every computation thoroughly checked. The high weights awarded to the Windsor observations in the cometary researches undertaken by German savants is the greatest compliment on his work and accuracy that Mr. Tebbutt could well receive. Mr. Tebbutt drew my attention to a fact that I have not seen noticed in works on astronomy. I was speaking to him about the angle from N. point in observing an occultation; he said that he took no notice of these angles, as all he had to do was to put the micrometer-wire near the circle of declination the star is situated on, for there it must both disappear and reappear. Climatic differences have now and then given rise to the necessity of corrections unknown in colder or less extreme climates. The thermometer often registers over 100° in the shade in summer and has even registered 117°, but Mr. Tebbut has taken every care to minimize the effects of this extreme heat on his transit observations.

The occasionally excessive rainfall, sometimes 20 inches in 4 days, has had the peculiar effect of altering irregularly the altitudes of the piers of the transit instrument, and on fine warm nights the mosquitoes are by no means to be despised when making a delicate observation.

The amount of work accomplished by Mr. Tebbutt shows what private observers can do if their enthusiasm is tempered with system.

The intense purity of the skies in Australia can hardly be conceived by people living near London. The colours assumed at the setting of the sun are very fine, a delicate rose tint as contrasted with the red in England, with often a few isolated pink streamers extending high into the heavens.

On clear dry days the sun shines with great force, so that even when not looking towards light people in the open air contort their faces on account of the glare.

The heavens generally are more striking than in the north, and no doubt as Australia becomes more settled observers will become frequent and share with Mr. Tebbutt the position he now holds alone.

In conclusion, perhaps you will allow me to use this opportunity of thanking Mr. Tebbutt for his genial hospitality to a stranger. His English welcome was peculiarly acceptable in strange surroundings. The picture of his home near his Observatory, in which he is quietly devoting his life to astronomy with his family round him, occupied my recollection very pleasantly on the return journey to Sydney. R. T. A. INNES.

Selenographical Notes.

VITELLO AND LEE.—All the large formations which figure on the border of the Mare Humorum possess characteristics that distinguish them from the ordinary type and render them and their surroundings peculiarly interesting both to the observer and the lunar theorist. On the north we have Gassendi, with its wonderful interior, rent in all directions by clefts, and exhibiting a central mountain of curious complexity: on the west, Agatharchides and Hippalus with their incomplete borders and remarkable rill-systems; on the east, Mersenius with its convex floor, and Doppelmayer—another example of a ring with a ruined wall: and, finally, on the north, Vitello and Lee, the first being one of the very few instances of a ring-plain within a ring-plain: and the last, a fourth example of the partial destruction, from causes unknown, of the side of a formation facing the Mare.

Excepting the representations in the maps of Mädler, Neison, and Schmidt, there are few published drawings of Vitello. The formation is comparatively small, and surrounded on every side except the north by an amount of detail which requires a much larger scale for its adequate representation than those of any of the above charts. Schmidt's map of it, except as regards the interior, is far from being the most successful example of his skill in lunar delineation, and this remark applies especially to his drawing of the region east of Vitello (including Lee) and the southern side of

Doppelmayer. Vitello shares with Taruntius the peculiarity of possessing an inner concentric ring; but this feature in the case of the latter is not complete, exhibiting a want of continuity in two or three places; while the interior rampart of Vitello has no apparent break, though its very close contiguity with a very massive and interesting central mountain, rising considerably above its summit, often prevents it from being seen in its entirety. Irrespective of this, however, it is by no means an easy object, to be viewed at any time when the central portion of the floor is free from shadow, for one often fails to glimpse it, even when conditions are to all appearance favourable and small details on the floor easily seen. This may be, and probably is, due to very slight changes in the angle of illumination. On January 21, this year, $9^h\ 0^m$ to $10^h\ 0^m$, a good view was obtained of it with a power of 350, the E. longitude of the morning terminator being about 50°. It was traced completely round the central mountain, and appeared to be of equal brightness throughout, except a narrow section of it, east of the centre, where it was crossed by a thin wedge-shaped dusky marking, extending from the then narrow shadow of the central mountain to the foot of the eastern wall. On this mountain two of Schmidt's craterlets were comparatively easy objects, but no traces were seen of the third, which he shows near the inner foot of the north wall, though I had a distinct view of it on November 28, 1884, at 8^h, under a lower Sun. Schmidt and Neison represent the border of Vitello as continuous, but at sunrise, and for some time after, there are clear indications of two narrow breaks in it on the north-west. These are prominently shown by Mädler, and one of them by Schroeter (T. LIV. fig. 2). On the opposite side, the crest of the south-eastern section of the wall is loftier than the south-western section, and, instead of being continuous with the latter on the south, appears to turn suddenly southwards and form part of the range of which Neison's β is one of the loftiest mountains. Two straight dark streaks were noted on the floor, one commencing at the *quasi* gap in the north-west border and forming a tangent to the northern side of the inner ring, and the other, nearly parallel to it, on the extreme southern side of the interior.

Between Vitello and Lee, but nearer the latter, there is a very peculiar circular formation, an evident depression in the plateau, bounded on the south and east by two prominent mountain masses. A bright little crater is the only object which breaks the monotony of its smooth interior, which is of a much lighter tone than the surrounding region. A remarkable rill-like valley, commencing on the west side of the more westerly of the two mountains just mentioned, winds round the southern and western side of this depression, gradually increasing in width and depth as it proceeds northwards, and being concentric with its circular margin. It ultimately cuts through the cliffs bordering the Mare west of Lee, and extends, I believe, as a delicate cleft some little distance on the surface of the Mare towards the point of junction of Doppelmayer and this formation. It is a very noteworthy object, resembling an old river-bed, and deserves attention, as hitherto it appears to have been unnoticed, or, at any rate, unmentioned. Schmidt shows only a few of the many isolated features visible at sunrise within the bay-like inflection called Lee, or on the site of its ruined wall. Their visibility is very ephemeral. On January 21st not a trace of one of them was apparent on the uniformly grey surface, with the exception of a crater near the middle of the opening, and a faint steeak traversing it proceeding from the northern end of the bay towards the place where the curved rill cuts through the cliffs.

THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1891, March 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Cassini's Division in Saturn's Ring.

GENTLEMEN,—

It is somewhat amusing to read, in M. Guillemin's recently published 'Les Planètes et leurs Satellites,' the statement in reference to the discovery of the principal division in Saturn's ring (note to p. 200), that "d'après les astronomes anglais, il paraît que dix ans plus tôt [than Cassini's discovery] (en 1665) l'astronome W. Ball aurait, à l'aide d'une excellente lunette de 38 pieds de distance focale, reconnu que l'anneau était double . . ." The misconception on which this was founded was, as is well known to all your readers, and doubtless to most astronomers, fully explained in 1882, so that the English works from which M. Guillemin derived the above statement must be at least nearly nine years old.

Yours faithfully,
W. T. LYNN.

Blackheath,
1891, March 18.

Celestial Spectrum Analysis.

GENTLEMEN,—

You have been so kind as to insert in the last number of the 'Observatory' a review of my book 'Die Spectralanalyse der Gestirne,' for which I desire to express my thanks; which are also due to Miss Clerke, who has praised my work in such flattering terms.

There are a few points which have called forth adverse criticism, in some cases Miss Clerke being certainly right, while in others she is I think wrong. However, I should not have mentioned them were there not *one* point of such high importance, that it is my duty to defend my opinion. Miss Clerke says: "Unfortunately, however, the wave-lengths are given in a Potsdam scale, the introduction of which is equally unnecessary and undesirable. Ångström's scale cannot, of course, be permanently retained, but Rowland's is its destined successor, and the multiplication of standards should be strenuously resisted."

Miss Clerke cannot have read my explanation of the reasons which decided my use of the Potsdam wave-lengths (page 170). I should like to hear upon what grounds Miss Clerke bases the inference that Rowland's tables are to be the successors of Ångström's. Upon ground, I fear, artistic rather than scientific.

There can be no doubt that Rowland's tables are finer than the Potsdam ones, nor that they are the best which exist, but unfortunately Prof. Rowland has not given a *catalogue of all his lines from exact measurement of the negatives.* The wave-lengths taken from the Rowland tables can be employed for many purposes, but not for all—for example, for the reduction of photographic stellar spectra they are not sufficiently complete. The Potsdam wave-lengths can be employed for *all* purposes; I know no other catalogue of such completeness. Moreover, the Rowland standards have been changed from time to time, and there is no publication which makes directly evident the method by which the Rowland standards have been calculated. They all depend on the wave-length of one single line. On the contrary the Potsdam calculations are accessible to everybody.

It is as if we compared a fine chart of the heavens and a meridian catalogue of stars. For orientation the chart is indispensable, and in some cases the positions taken from it suffice, but in many others they do not, and then we must have the catalogue. In practice exact values of wave-lengths are much more useful than the best reproductions of spectra, and I believe that if Miss Clerke had to deal with the measurement and reduction of good photographic stellar, or other, spectra, she would use Rowland's tables for orientation, but when it came to reduction she would say "Give me the Potsdam Catalogue." And thus my opinion is, as already expressed in my book, use both—one for orientation, the other for calculation. Since the constant differences between Rowland and Potsdam are small, there is no difficulty in finding Rowland's lines by Potsdam wave-lengths.

Therefore I must say, that if I had to write my book again I should again use the Potsdam wave-lengths. I should be glad if this discussion could contribute to the propagation of the Potsdam wave-lengths among practical workers in spectrum analysis, so that "the multiplication of standards might be strenuously resisted."

Yours faithfully,

Potsdam, 1891, Feb. 28. Dr. J. SCHEINER.

The Perseid Radiant.

Gentlemen,—

Recent articles which have appeared in the astronomical journals in relation to the Perseid Radiant have caused a deeper tone of interest to be attached to any reliable meteor work that has been done during the early part of the month of August. Those who are interested in meteoric astronomy will hail with pleasure the list of 418 meteor-paths which appear in No. 3016 of the 'Astronomische Nachrichten.' They are observations made at Warendorf in the year 1888, chiefly on August 8, 9, and 10, by Plassmann and others.

I have made an independent examination of these paths for the purpose of sorting out the Perseid meteors. The results are interesting, as they in part bear out Mr. Denning's conclusions, and add weight to his discovery of the shifting of the radiant of these August Perseids. My conclusions are as follows:—

Date.	Radiant Point.		No. of Meteors.
August 8th......	41°	+52°	21
August 9th......	44	+55	28
August 10th......	46	+53	65

It is noteworthy that these collective meteor-paths give a radiant for August 8th very near to that found by the independent observations of the writer and Mr. Denning for the same date in the year 1888. The R.A. of the radiant is the same in each instance, but the Decl. as found from my discussion of Plassmann's list is 5° south of that found by myself and Mr. Denning from our separate observations on this date.

The diffuse character recorded at Leeds and Bristol for the Perseid radiant in 1888 is also very prominently marked from Plassmann's list; but the centre of radiation is very clearly shown, although the diffusion extends over several degrees both in R.A. and Decl.

Yours faithfully,
David Booth.

70 North Street, Leeds,
1891, March 14.

Early Comets.

Gentlemen,—

For the completion of a good catalogue of Comets such as that in Chambers's work a few additional entries still seem necessary, as *e. g.*:—

A.D. 541. "A comet appeared in Gaul so bright that the whole heaven appeared to be on fire" (Mat. of Westm.).

A.D. 906. "A comet appeared and lasted nearly half the year" (Mat. of Westm.). The length of time shows that this is quite distinct from the Chinese observation of 905, for that comet was seen for less than a month.

A.D. 1077. "A blazing star appeared on Palm Sunday, being 16th April, about six of the clock, when the air was fair and clear"

(Hollinshed's Chronicles). It is difficult to see to what the reference can be, except to a comet, as no new star is recorded.

One is almost disposed to refer the following description to a comet:—

A.D. 849. "A pillar of light rose up to heaven and was visible for thirty days." This is very unlike any reference to an aurora.

Yours faithfully,

S. J. Johnson.

Melplash Vicarage, Dorset,
1891, March 10.

OBSERVATORIES.

Paris.—Volume xix. of the 'Annales,' dated 1889, is a collection of Memoirs of which several, if not all, have already appeared in a separate form. The first, by M. Périgaud, is a detailed account of the method used for determining the division errors of a Circle. The second is a contribution to planetary theory by M. Boquet, being the calculation of the eighth term of the disturbing function. M. Tisserand has shown how essential this term is to the theory of planets like Pallas, whose orbits are of more than average eccentricity and inclination. The third paper is on personality in measurements of double stars by M. Bigourdan. He recommends as the most satisfactory method the use of an artificial pair of stars which can be set to known distances and position-angles, whose magnitudes can be altered at will, and the images of which can be disturbed and diffused by a gas-flame so as to give all the disadvantages of celestial observations; and describes in detail an apparatus for the purpose by which he determined his own personality.

Then follow the Comet observations made from 1835–1855. During this period 39 Comets were observed, but the results have never yet been published; and M. Bossert has now charged himself with the publication. Two memoirs by M. Callandreau and one by M. Henry on the figures of the heavenly bodies; M. Radau's essay on refraction; and a short paper on the stability of the Solar system by M. Eginitis complete an important and interesting volume. The results of the last paper may perhaps be quoted in full:—

1. The major axes of the planetary orbits are subject to secular inequalities of the third order in the disturbing masses.

2. The inequalities are periodic and of very long period, so that for several centuries they may be considered to vary directly as the time.

3. The Earth and Saturn in particular are at present very slowly approaching the Sun.

We notice at the same time the appearance of an extract from volume xx. of the 'Annales,' viz. M. Callandreau's memoir 'Sur la théorie des Comètes périodiques,' in which he continues M. Tisserand's researches on the capture of comets by the major planets of our system.

PUBLICATIONS.

Astronomical Society of the Pacific.—Number 14 of the Publications contains much of interest. Prof. Holden gives an account of his observations of Saturn 1879–89, with illustrations. The following sentences are important in connection with Prof. Asaph Hall's very similar conclusions noted recently:—"The observations were all made with unusually powerful telescopes. A particular feature in regard to the series is that the sketches do *not* show certain peculiarities of *Saturn*, which have been reported by other observers with smaller telescopes."

There is a useful index-map of the Moon contributed by Prof. C. A. Young; and an account of the Observatory of Swarthmore College, by the Director, Miss S. J. Cunningham. After some other papers, follow the now well-established "Notices from the Lick Observatory, prepared by members of the Staff." In one of these, on the Satellites of Mars, we read:—"During April and May (1890) two observers made a conscientious search for new satellites. The weather conditions were rather unfavourable. The general conclusions reached were that no new satellite exists within the orbit of *Deimos*, which is anything like as bright as one fourth the brightness of that satellite. It is possible, though not very likely, that so faint a satellite as this may exist outside of Deimos' orbit or within that of *Phobos*." We are glad to learn also that Dr. Otto Dziobek's planetary theory is to be translated into English by Prof. Harrington, of the Michigan Observatory. The work is to be issued to subscribers at $3.50 (orders can be sent direct to Prof. Harrington). Mr. Newall's visit to Mt. Hamilton is recorded, and it is announced that Lord Rosse and Dr. Auwers are expected there soon.

Prof. Holden devotes a final note to the expression of a hope that the U.S. Naval Observatory may be furnished with a new Constitution when the establishment is transferred to the fine new buildings now nearly completed. We drew attention in our last number to the Report of the Superintendent on the inadequacy of the Staff.

A Catalogue of Binary Stars.—This is a reprint of a paper read before the Royal Irish Academy in June 1890, and is of considerably more value than its title indicates. Mr. Gore has catalogued 61 pairs for which orbits have been computed. They are arranged in order of R.A. (1890·0). The R.A. dec., P., T., e, i, Ω, λ, a, the computer and date of computation follow in columns. Then we have the magnitudes, colours, hypothetical parallax, observed parallax, values for A and B in Rambaut's formulæ, relative brightness, and references to original work. All this information can be seen at a glance by persons working on theories in connection with the construction of sidereal systems, and much time and labour saved thereby. To make the table still more valuable, Mr. Gore has given copious notes to each pair, with recent measures. It seems ungracious to complain, but if Mr. Gore could have managed to insert an extra leaf in the reprint giving more recent notes, it would have added to its value. We have in mind Schorr's work on ξ Scorpii (Σ 1998), analogous to Seeliger's ζ Cancri. By the way, there is an unfortunate misprint in the note to this system, it should be ζ Cancri and not ξ. We do not know why Struve's magnitudes are preferred to some more recent authority.

COTTAM'S CHARTS OF THE CONSTELLATIONS *.—In no. 158, p. 57, we referred to the first edition of Mr. Cottam's charts. We have now received an edition reduced to 12 × 15 inches, and accompanied by descriptive letterpress, the whole being serviceably bound. Our favourable comments on the first edition are applicable to this also, which is perhaps more convenient in size for practical work. We can heartily recommend those who desire a good atlas to obtain Mr. Cottam's work.

NOTES.

METEOROLOGY IN THE 'TIMES.'—It would seem that the practice has hitherto been to give the barometric indication for the 24 hours ending 2^h A.M. of the day of publication, as well also as information on temperature, dew-point, &c., for noon, 9^h P.M., and 2^h A.M. of the same 24 hours. In this way one day weekly (Saturday) becomes altogether omitted.

Now the practice apparently is to give the barometric information on Monday for 48 hours, by which the record is made continuous, giving on other days the record for 24 hours as before. But the contracted time-scale on Monday is inconvenient, as causing the curve to the eye to have a greater steepness as compared with other days. It might be well to give the record *on every day* for the previous 48 hours; the diagrams on different days would then be strictly comparative, besides which such information for 48 hours is not too much to include.

What has been said regarding the barometer applies also to the "Temperature and Hygrometric condition." One day weekly is omitted. This information might also be given on Monday for 48 hours to make it continuous, as for the barometer. And if the barometric information should in future be given on every day for 48 hours, then the "Temperature and Hygrometric condition" might similarly be given always for 48 hours.

The "Temperature &c." condition is said to be for "London." It does not seem clear whether this is the 'Times' office or elsewhere.

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* E. Stanford, 26 & 27 Cockspur Street, S.W.

THE SOLAR SPECTRUM AT MEDIUM AND LOW ALTITUDES.—A very important study of the telluric spectrum on a large scale has recently been published by Dr. L. Becker, of the Royal Observatory, Edinburgh. The instrument used was a grating spectroscope fitted with an ingenious recording instrument devised by Dr. Becker, by the aid of which he was enabled to record the places and intensities of 3637 solar lines in the district λ 6024 to F. The object of the investigation being especially the study of the telluric lines, the spectrum had to be very carefully observed with the Sun at various altitudes, and 928 lines are recorded as due to our own atmosphere. These are mostly arranged in three bands; the first running from λ 6020 to λ 5666, and containing 678 lines. The other two correspond to bands ζ and ι of the Brewster-Gladstone spectrum, and contain 106 and 110 lines respectively.

The maps, which are very carefully drawn and have been well reproduced by photolithography, are designed on a scale of oscillation frequencies, a scale which the late Astronomer Royal for Scotland strongly advocated, and which has some decided advantages. A scale of wave-lengths is also supplied, and renders yet more complete a work of very high value and importance to all interested in the study of the spectrum.

EMILE GAUTIER.—We regret to record the death of Colonel E. Gautier, who succeeded Plantamour as Director of the Geneva Observatory in 1882, and held the office for seven years. Born at Geneva on 1822, April 18, he studied under Le Verrier, assisting in the calculations which led to the discovery of Neptune. In 1847 he published an "Essai sur la Théorie des Perturbations des Comètes." In 1860 he observed the total solar eclipse in Spain. Despite active military duties he from time to time contributed to astronomical publications. In later years he was assistant astronomer at Geneva, and during the illness of Plantamour virtually filled the post of director.

MINOR PLANETS.—M. Charlois has named the following:—No. 296 Phaëtusa, No. 297 Cecilia, No. 298 Baptistina, No. 300 Geraldina, No. 302 Clarissa. M. Millosewich discovered No. 307 on March 1, and M. Charlois No. 308 on March 5.

WHEN NIGHT BEGINS.—From time to time the question of what constitutes night has arisen in courts of law in this country, and it appears from the following note by the Paris correspondent of the 'Daily Telegraph' that the question has also arisen in France.

He says :—"There has been considerable discussion lately in one of the provincial law-courts as to the precise definition of the word 'night.' A prisoner was being tried on the charge of having broken a law nearly half a century old, which sets forth that no sportsman shall be permitted to indulge in his favourite pastime during the nocturnal hours. The gendarmes who had discovered him blazing away in a wood had drawn up a *procès-verbal* in due form, and they were much disconcerted when it was contended on behalf of the defendant that they had been completely in error, and that the day was not yet over when they arrested him. This raised the question as to the precise moment at which night might be said to begin officially, and there was much reference to scientific works. Finally it was decided that night might be regarded as having commenced with the close of the twilight—that is to say, when the Sun had descended more than 6° below the horizon. The 'Annuaire du Bureau des Longitudes' was then consulted, and it was ascertained that twilight had ended on the day in question thirty-seven minutes after sunset in the wood in which the sportsman was apprehended, and as he had been detected in the act of firing a shot after that time he was found guilty and sentenced to the penalty prescribed by the law. So sportsmen are now warned that they must not prolong their amusement after the Sun has descended 6° below the horizon, and if they are inclined to be very accurate they must provide themselves with proper instruments for the purpose of ascertaining the fact with the requisite precision."

M. LOEWY'S WORK ON ABERRATION.—We are glad to see in the 'Comptes Rendus' for March 16 a note by MM. Loewy and Puiseux on their determination of the Constant of Aberration. The main part of the note is an account of the circumstances which have during recent years caused some doubt to be thrown on the accuracy of Struve's constant 20″·44, published in 1843; and the results of M. Loewy's work for 10 months are given in three important sentences at the end as follows :—

"1. Struve's value 20″·445 is very near the truth. It would, in our opinion, be premature to alter it.

"2. M. Fizeau's result, that reflection does not affect the behaviour of rays with regard to aberration, is confirmed.

"3. The new method for determining aberration can be regarded as satisfactory and definitive."

The experiments are still in progress, and details of those already made and those to be made will be given in a future memoir; but the importance of the work is well established by this preliminary note.

THE POSITION OF THE NEBULAR LINE.—Dr. Huggins communicated to the Royal Society on March 13 an account of Mr. Keeler's work at the Lick Observatory, on the "Chief Line in the Spectrum of the Nebulæ." One of the most interesting points brought out in the paper is the astonishing accuracy of the measures with the fine instrument of the Lick Observatory. A discordance of 0·15 tenth-metre on one day was immediately suspected as due to some systematic error; and on examination the telescope of the spectroscope was found to be 5° wrong in position-angle, which adequately explained the discrepancy. Mr. Keeler says that he would "with some confidence undertake to determine the month of the year, by measuring the distance of the principal line (in the spectrum of the nebula of Orion) from the lead-line used in the comparison spectrum."

Results based on measures so accurate as these must be received with great respect; and we quote verbatim the conclusions at the end of the paper :—

"(1) The nebular line is 0·43 tenth-metre more refrangible than the lower edge of the magnesium fluting.

"(2) The nebular line has no resemblance to a fluting.

"(3) Flutings and lines of magnesium, which could not fail to appear at the same time with the fluting at λ 5006·36, are entirely absent in nebular spectra."

It appears to Mr. Keeler "that the non-coincidence of the chief nebular line and the magnesium fluting must be regarded as proved," and that thus Mr. Lockyer's deductions from the assumed coincidence do not hold good.

LOGARITHM TABLES.—A magnificent volume has just been issued by the Paris Government (Service Géographique de l'Armée), entitled 'Tables des logarithmes à huit décimales des nombres entiers de 1 à 120,000 et des sinus et tangentes de dix secondes en dix secondes d'arc dans le système de la division Centesimale du quadrant, publiées par ordre du Ministre de la Guerre.' There are about 700 pages, each 14½ in. by 11 in., the type being very clear. One cannot but regret that the latter part of the work is cast in the Centesimal system, in which it is useless to all but French geodesists.

PERHAPS I may be allowed to dissociate myself from my colleague, Mr. Common, so far as to congratulate him on the honour which is to be conferred on him on April 16, when the University of St. Andrews are to create him LL.D. Our readers will, no doubt, be glad to hear of this graceful recognition of Astronomy, in the person of one of its devotees. H. H. TURNER.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 175. MAY. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, April 10, 1891.

Lieut.-Gen. J. F. TENNANT, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. KNOBEL and A. M. W. DOWNING, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Chambers. May I ask why the 'Monthly Notices' for March have not yet been issued?

Mr. Knobel. There is no real necessity to issue the 'Monthly Notices' before the meetings of the Society, and in this case it could not have been done without sacrifice of accuracy.

Mr. Downing. Sixty-five presents have been received since the last Meeting; amongst those calling for special mention are:— 'Annales de l'Observatoire de Paris,' Mémoires, tome xix., presented by the Observatory, and 'Œuvres Complètes de Laplace,' tome viii., presented by the Academy of Sciences, Paris.

The President. In expressing thanks for the presents there is one subject I should like to bring before the Society, and that is that the observations of Loewy and Puiseux have been provisionally reduced, and we shall have the results shortly. You will be glad to hear that in the "Note on Aberration," read before the French Academy, the values given entirely confirm those of Struve.

Mr. Knobel read a paper by *Mr. S. W. Burnham* on "Invisible Double Stars." Some of the invisible double stars are recognized from certain irregularities in the movements of known bright stars, others are recognized by the periodical doubling of the lines in their spectra, while a third kind is recognized from changes in magnitude of bright stars. The difficulties of eliminating errors of observation in the first class must cause the results for these stars to be received with caution, while for the spectroscopic doubles there has as yet been no confirmation of the results of the observations. In dealing with those stars that are supposed to be double, because of the apparent irregularities in their movements, we must not place too much reliance on the observations. Taking the case of ζ Cancri, the star of this class that has received most attention, I have endeavoured to lay down all the observations accurately to scale, but without success. It has been assumed that Sirius has a dark attendant; but here again the observations cannot be plotted with any accuracy, and the same applies to all the more remarkable stars of the same kind. All the stars I have examined in this way have such small irregularities in their motions that the probabilities of error are often greater than the irregularities, and this being so it is not at all extraordinary that the observations should show apparent agreements in position at irregular periods of time.

Captain Noble. Unless these invisible stars are absolutely devoid of light, photography might assist us here, for it is only a question of prolonged exposure to get the image of any star which emits light.

Mr. Ranyard. In the case of Sirius the light of the bright star would cover up the faint dark companion. It has always been difficult for me to believe that a star which emits only one forty-thousandth of the light of Sirius should have half the mass of the bright star.

Mr. Downing. It seems to me the weakest point in Mr. Burnham's paper is the regularity of the intervals of time at which the discordances of position occur. When we have these discordances occurring over and over again at regular intervals there is really something like a *primâ-facie* case made out for the unseen companion. Mr. Lewis has given considerable attention to this point, and I am sorry he is not here to speak, but to-day he mentioned one star of this class in which there were eleven such discordances at regular intervals.

A vote of thanks was accorded to Mr. Burnham for his paper.

Mr. Turner read a paper by *Colonel Bassot* and *Commandant Defforges* "On the recent determination of the Longitude of Paris—Greenwich," and a reply by himself.

Mr. Turner said that in a paper read in January he had mentioned a discordance between the results obtained by the English and the French observers. Four observers took part in the work, two English and two French, and the signals were satisfactory all through; but it was found that the time determinations at the two ends did not agree, in fact the observers at Paris had apparently a different personal equation when they came to Greenwich, and on their return to Paris their original personal equation was restored. In connection with this he had, in a former paper, recorded a fact and an opinion. The fact was that after a very careful search through the reductions nothing was found to throw light upon the subject except one matter, viz. that there was a curious difference between the nadir and the striding-level determinations of level error in the case of the French instrument used at Greenwich. Having noticed that one of these (the nadir observations) would give accordant results, he appeared to have stated too strongly his opinion that the other determinations were wrong. The French observers in their present paper called attention to this as only an opinion, and they maintained that the proper course was to take the mean of the two methods of determination as they had done. They also point out that the personal equation of the English observers varied during the operations, as was natural, seeing that they were new to the small instruments used, although familiar with the transit-circle; and it was true enough that any change of personal equation naturally leads to suspicion with regard to results. Those were the main points of the paper which the French observers had communicated to the Society. They certainly strengthened their position when they called attention to the very large accidental error that appeared in the comparisons of the results with the transit-circle observations at Greenwich, and there could be no doubt that the probable error of such a comparison was large, larger perhaps than had been supposed. The determination of time was not such a simple matter as it would appear at first sight, for determinations with two or three instruments side by side did not give such accordant results as we should have expected. The French observers considered that these differences were so very large that the comparison could not be used to justify one set of results and to condemn another. They also objected to the comparison with the transit-circle on the ground that there was interposed between the transit-circle observations and the longitude ones an additional relay, and they suspect that this was not quite constant in its action. The relay was tested on various occasions, and never found wanting, so that he (Mr. Turner) did not think there were any grounds for looking on it with suspicion. And, again, the relay had nothing to do with this particular point, since it was common to both instruments at Greenwich, and could not, therefore, have produced any differential effect. The main point of his reply was to call attention again to the fact that the series of longitudes could be brought into agreement by the simple supposition that the nadir determinations of level error were correct.

The Astronomer Royal. The Fellows present may perhaps think these matters are rather technical, but they are very important, and these discussions will assist us very much in longitude determinations in the future. It is on this account that I welcome the opinions of the French observers. I hope that next year, in concert with them, we shall be able to have a new determination which I trust will give more accordant and trustworthy results. Various sources of error have been brought to light in these discussions, and will be guarded against in the future. One point is with regard to the relay. We carefully discussed our programme with the French observers before commencing work, but we overlooked the point that while we work with *make* contacts to our chronographs the French use *break* contacts. As soon as the French observers came over we found that in order to work their chronographs it was necessary to interpose a relay. I have been discussing the time question with Commandant Defforges and we have come to the conclusion that it would be better for each set of observers to have their own clocks. Our difficulties will, it is hoped, be overcome in this way. I agree with Mr. Turner that the discordance between the transit-circle and the longitude observations can hardly be explained by the variations in the action of the relay, but still there is a possibility of error from that cause.

The President. I would like to know whether next year there is any hope that you will be able to determine the longitude directly between the observatories of Paris and Greenwich.

The Astronomer Royal. In my opinion, and I think in the opinion of the French authorities, the Observatory of Montsouris is practically the zero with which we ought to compare. The French Geodetic Survey uses this as a starting point for their determinations of longitude, and it is of more interest to join up with this Survey than to determine the exact longitude of the Paris Observatory. In England the Geodetic part of the Trigonometrical Survey was practically finished some years ago, whereas in France it is still in hand, so that the circumstances in the two countries are somewhat different.

The President. I think we may give a very cordial vote of thanks to Colonel Bassot and Commandant Defforges for their paper, and to Mr. Turner for his kindness in trying to elucidate this matter. I am sure we all hope that next year there will not be the differences between the results that were found at the last determination of this longitude.

The Astronomer Royal has just returned from Paris, and I will now ask him to give us some account of what passed at the Congress there.

The Astronomer Royal. I have not prepared any report on the proceedings, but a general statement of what went on may be of interest. In the first place, I may mention that a Sub-Committee prepared a programme of the questions to be discussed at the Conference. I will not detain the Fellows by reading the actual questions proposed, because some of them were passed over and others amplified considerably. In the first place, the Committee received reports from the various Observatories on the installations of their photographic telescopes, and I think, with one or two exceptions, the installations were reported to be complete. Unfortunately the war now going on in Chili will for some time prevent the observatory at Santiago from taking part in the work. Specimens of photographs were submitted by a number of observatories, and a Sub-Committee, of which Mr. Plummer was a member, was appointed to examine them. I believe the Committee arrived at the conclusion that the photographs were remarkably uniform in their character, considering that different makers of instruments and different photographic plates were used in different countries. On the whole, very good results were exhibited. Reports were also received on the preparations made for the catalogues of guiding stars. These are in an advanced state in most cases. After that came discussions on various questions which were submitted to the Committee. The first resolution was that the guiding stars might be chosen at a distance up to 40′ from the centre of the field. The next point, I believe, was as to how the *réseaux* were to be imprinted, the Committee having decided at a previous meeting that they should be imprinted by parallel rays formed by the photographic objectives. As a result of the representations made by one or two members, it was decided to allow the impression of these *réseaux* to be made by means of parallel rays either from refractors or reflectors. Another point was that the plates were to be oriented for the epoch 1900 in the zones from 65° of the pole. The next question gave rise to considerable discussion, and that was whether the execution of the chart of the heavens should be postponed indefinitely till the plates which were to be taken for the Catalogue were completed. It was urged by many of the members that whilst we had all come to definite conclusions as to the conditions under which the Catalogue plates were to be taken, there was considerable diversity of opinion as to the length of exposure for the Chart plates. In the end it was agreed that the Committee, whilst recommending that the execution of the plates for the Catalogue should be pushed on as actively as possible, were of opinion that advantage should be taken of the greatest number of fine nights for the long-exposure plates of the Chart, so that the two classes of work will go on concurrently. Another question brought forward was as to the length of exposure for the Catalogue plates. At the last meeting of the Committee it was decided that two exposures should be given to the plates for the Catalogue stars. One was defined to give stars of magnitude 11, and another exposure of one fourth the duration was to be given with a small displacement of about 15″ or 20″, the object being to be able to identify readily stars given by the long exposure. It was decided to lengthen the short exposure and to make it one half of the longer, so that it would show *faintly* stars of magnitude 11. A further question arose as to how the 11th magnitude was to be defined, and some criticism was offered on the two alternative definitions which had been laid down by the Committee two years ago. One of those was that the exposure was to be for the Catalogue plates 6¼ times that required to give the 9th magnitude of Argelander; but the following resolution said that each Director was to assure himself that the plates which he took for the Catalogue contained the 11th magnitude stars as found by calculation from the 9th magnitude with the coefficient 2·512, which is the usual coefficient for passing from one magnitude to another. The two definitions would correspond on the assumption that the time of exposure varies inversely as the brightness of the stars to be photographed; but it was felt, in view of some remarkable results brought forward by Dr. Scheiner, that these two definitions were not necessarily identical, and in order to avoid that difficulty, a resolution was passed that the 11th magnitude was to be determined from the 9th magnitude by means of a wire-gauze screen placed before the object-glass.

Another question which gave rise to a very animated discussion was whether it would not be better to give a triple exposure in the form of a close triangle, so that the images would blend together, and when the plates were reproduced, whether by photography or by any other method, the images would form a distinctly visible spot of light which could be distinguished from specks of dust or any accidental defects. Finally it was arranged that the first series of plates for the Chart (centres at even degrees of declination) were to be taken with a single exposure. Further researches were to decide whether the Committee would recommend two or three exposures instead of one for the duplicate series.

Then came the question of exposure for the Chart plates to give magnitude 14, which practically involved the definition of magnitude 14 photographically. Some members naturally wanted to have the exposure as long as possible, and others not too long, and nobody could really say what exposure was required for the 11th magnitude, to serve as a basis for defining the length of exposure for the 14th on the chart plates. Finally, we determined to adopt a resolution to the effect that the exposure for the first series of chart plates was to be 40 minutes under mean atmospheric conditions at Paris and with the Lumière plates now in use there, and that the exposure (in minutes) at any other observatory should be $40^{m} \times t'/t$, where t and t' are the times (in minutes) required to give 11th magnitude at Paris and the other observatory respectively.

Another point of importance was the selection of the reference stars in each plate and arrangements for determining their positions. Of course when the plate is taken it is important that we should have all the elements for determining the positions of the stars on it. For that purpose a Sub-Committee was appointed to hold a meeting on April 4, and it was agreed as a general proposition that on each plate there should be about 6 stars, the positions of which were to be well determined by meridian observations. Thus it would be necessary to form a catalogue of 60,000 to 70,000 stars from meridian observations to be made within the next few years. It seems very desirable that that should be carried out, and the Committee agreed on the basis, and the details will be settled by further correspondence.

No resolution had been come to as to the time when the work of the photographic map should be really begun, but it was decided unanimously that the work should be begun as soon as the wire-gauze screens have been supplied to the various observatories for determining the 11th magnitude. That, perhaps, will take about two or three months. There was a strong feeling among French astronomers that in the region of the heavens in which the asteroids are found, the charts should go beyond the 14th magnitude. The Committee decided that that was rather beyond their functions. The final resolution was on the proposal for the redistribution of the zones assigned to the observatories. A new plan had been proposed as a modification of that adopted at the last meeting of the Committee. There were naturally some objections raised to that. Observers who found they were working either too near or too far from the zenith did not like the arrangement exactly ; but as no one suggested any alternative, the arrangement proposed was adopted. I feel sure all present will agree with me that the meeting this year has put on a practical basis this great work of a photographic survey of the heavens. It has now been started under practical conditions, and I trust it will go on without further hitch. Any future developments may easily be discussed without interfering with the general scope of the work.

Captain Abney. The Astronomer Royal has covered nearly all the ground, and has left me very little to say. One point to which he alluded to I should like to refer to, and that is, how the map is to be reproduced. I think there was a general feeling at the Congress that it would be unworthy of the map if it were produced by anything except photography, and that the most delicate tool—light—should replace the pantograph. Regarding the proposal to make three exposures, I think the principal argument in its support depended upon the idea that the maps were to be on albuminized paper, and were to be white spots on a black surface. There is a great difference in recognizing stars shown as white spots on a black surface compared with black spots on a white surface. Anyone who examines a star photograph under a microscope can easily tell a star from a speck of dust, and for this reason I think three exposures are not necessary. Finally it was settled that the map is to be reproduced by photography, but not by any process which would be liable to deterioration by shrinkage in the medium on which the stars are shown. It would be premature for me to say how this is to be done, because I am on the Committee appointed to decide this, though I have a definite opinion on the matter. The great variety of plates used for the map was very striking. They were of all kinds—good and indifferent. Some showed the parsimony of the plate makers, for the films were too thin, and a great deal of the light passed through and was reflected back, forming a ring round the star. Other plates did not show this, but were sharp and perfectly satisfactory in this respect. I think the great question for the observers to settle before they set to work is to find the most sensitive and suitable kind of plate. I think I can indicate a formula for the preparation of the plates that will give far better results than will be obtained by going to ordinary plate manufacturers, who study their pockets more than science. The Astronomer Royal has, I think, given a very graphic account of the proceedings of the Congress, except perhaps that he omitted recounting certain very earnest debates that rendered the proceedings anything but monotonous.

Mr. Plummer. I have the disadvantage of speaking after two great authorities, and possibly therefore the only point to which I can refer with advantage is the examination of the plates, of which Committee I was a member. That Committee consisted of the Brothers Henry, Dr. Scheiner, and myself. We went through the plates from the various observatories with a great deal of care. The points to which we paid attention were the character of the field, the quality of the image, and the distance from the centre of the plate at which those images were measurable and the position of the plate with regard to the focal plane. The result at which we arrived was the general similarity of the whole character of the plates, whether they had been made by one firm or another. It had been urged in the early days of the photographic chart that it was necessary, or at least desirable, that one firm of artists should be entrusted with the construction of the object-glasses. But just as with the visual object-glass there is no great superiority if you go to an efficient artist, so in the case of photographic object-glasses the result was equally satisfactory. Of course there were some rather bizarre negatives shown. One gave an image of ϵ Orionis which had been exposed eight hours. I think I am not exaggerating when I say it was half an inch in diameter. A matter of regret, to my mind, was the fact that no negatives from the Paris Observatory were exhibited; but I have no doubt from what I have seen elsewhere that their object-glass is quite comparable with those we had the opportunity of examining.

Capt. Noble. Can you induce Mr. Roberts to say a few words on the subject? I had a most interesting discussion with him in the Council-room, in which he gave some remarkable results of his on the effect of exposure—how one night he got only 40 stars when on another night he got hundreds.

Mr. Roberts. I do not think any remarks I may make will have great weight, for this reason. The chart is to be prepared with instruments differently constructed from mine, with a smaller aperture and of relatively longer focal length, and the differences to which Captain Noble has referred may not be so pronounced with that sort of instrument as they are with mine. It is quite true that I have taken several photographs of the same stellar area on nights of different quality, and that I have obtained on what appeared to be a bad night with a hazy sky and a good deal of moonlight, with 15^m exposure, 400 stars, and that either on the same night or one following, with an exposure for the same length of time, I could not get a fifth of the number of stars. On a night when the sky was fairly clear and the exposure 60^m, the number of stars was under 200, whereas on another apparently bad night, with an exposure of 15^m, I got a much greater number.

These are only indications that great uncertainties have to be dealt with in determining the required exposure in charting the stars, but they will be met as they arise by those who will be engaged in the photographic work; by degrees we shall find out the cause of these extraordinary differences, and then they can be dealt with and some intelligible system of exposure adopted.

Capt. Abney. It is perfectly possible that on luminous nights an action has taken place on the plate at the same time that the stars are being impressed. In Switzerland I have taken some photographs of stars, and certainly when we had no Moon and a very black sky we always got fewer stars on the plate. The light from the sky was doing its work when the stars were impressed on the plate.

Mr. Roberts. I thought the moonlight had something to do with it, but I expected the result to be uniform when the moonlight was on; but I did not find that to be so.

Mr. Turner. From what the Astronomer Royal has said it seems to me the Committee have been somewhat in the position of those gentlemen in the Play who, after having been detected in various falsehoods, determined on one occasion to tell the truth; but after a moment's consideration, they asked—"What is the truth to be?" The Committee seem to have been, in the case of one of the resolutions, rather frightened at their own decision and to have reopened a question I thought settled, viz. whether length of exposure is to be considered equivalent to brightness. I should like to hear Capt. Abney repeat the statement I have heard him make, which it seems to me the world in general has not quite sufficiently accepted, and that is, that to increase the intensity is exactly equivalent to increasing the time for photographic exposure. Capt. Abney has raised another question, on which I shall be glad if he will say a word or two more, and that is as regards the initial action of faint light. Would he recommend that the plates should be exposed to a faint light first to start the action?

The Astronomer Royal. I might ask Captain Abney whether he would suggest that while the plate is being exposed there should be some scattered light which might come through the object-glass.

Capt. Abney. I have said before in this room that exposure and intensity are equivalent to one another—that is to say, that doubling the exposure is equal to doubling the intensity of the light, but I always put in a saving clause, "within limits." The limit is one which may have been arrived at. I have made careful experiments in regard to it. Where you use an aperture of lens, say, the 300th of an inch, and in another case an aperture of a quarter of an inch, they have a definite ratio to one another; but if you expose plates for times equivalent to those apertures, you find there is a very slight variation between the two. But that is only when you come to very minute intensities of light. You quite see how that can be. I do not think in the exposure to which these plates will have to be put that this limitation ought to come in. I believe that practically 2·5 times the exposures ought to give you what you require; that is to say, another magnitude. As regards the preliminary exposure, I think it would be very much better to get a more sensitive plate which does not require a preliminary exposure. As a rule, I believe that nearly every quick plate that is sold has had a preliminary exposure or, what is equivalent, a preliminary decomposition of silver salts, that is to say, that the manufacturer has fogged it unwittingly. A plate 15 on the sensitometer may be made to show 23 by a preliminary exposure. When you come to estimate star magnitudes you must recollect you are altering entirely the ratio of your densities and disks when you give this preliminary exposure. At the Congress M. Trépied had collected the various formulæ found to give an increase of one in the star magnitudes, and my own belief is that the differences in these formulæ were due to the fact that the plates from which they were obtained had had preliminary exposure or preliminary decomposition. A great many of the plates that were there were not bright plates; it might have been due to the bright sky to which they were exposed, but I could not detect that it was so, as the margins of the plates had the same veil, and they ought not to have had this if it were due to the bright sky. If you want to get out a great many stars, and do not care about their magnitudes particularly, it would be very advisable to give a preliminary exposure, but if you wish to measure the magnitudes do not do it. It is better to have a standard, and the only standard is a plate that is perfectly bright.

Mr. Plummer. It was distinctly stated by one of the experts present that the ratio of 2·5 did not correspond to successive increments of magnitude, and I have no doubt that that opinion exercised considerable weight with the Committee. I understood the Potsdam representative to say that up to a certain point it represented very fairly bright stars; but that for faint stars the time of exposure required to be continually increased, and that $2\frac{1}{2}$ times exposure sometimes gave $\frac{7}{10}$ magnitude, and sometimes only half a magnitude.

Capt. Abney. That seemed to be only experienced at the Potsdam observatory; but it seemed to me an extraordinary occurrence.

Mr. Ranyard. Here is a very curious point with regard to the printing of small stars. I find in producing star-maps with a very slight print you get very many more stars than when you print deep. Therefore in reproducing maps there ought to be some constant of the exposure of photographs even after they are made.

The Astronomer Royal. With regard to the reopening of the question of estimated magnitude, I did not see that it did any harm, and I thought the adoption of the screen gave a very valuable means of verifying the truth of that law. I thought the screen should be applied to test the law that the brightness of stars photographed varied inversely as to the length of the exposure. There was a feeling in the minds of some members that the photographic and photometric definitions were not necessarily identical; therefore I thought there would be no harm in adopting the photometric method.

Capt. Abney. Anybody who has attempted to measure the disk of a star will know that it is rather a shaky thing to do. It is very difficult so say where the disk extends to and where it does not, and I quite understand that if Mr. Christie and myself were to measure a star-disk we should differ. The same man should do it throughout. There is a method, however, I think by which a star-disk can be measured with the greatest accuracy. (The speaker then illustrated his method on the blackboard. See note at the end of this No.)

The President. I will ask you to return your thanks to those who have taken part in the discussion. We have not only had what passed at the Paris Conference, but we have had a good many comments on it which are very instructive, and I trust they will be well reported so that we can see them at our leisure.

Mr. Thackeray read a short note from his paper on "A Comparison of the North Polar Distances of the 'Nautical Almanac' for 1880 with the Cape Catalogue, the Greenwich Ten-year Catalogue, and Boss' Standard Star-places for 1880.

A vote of thanks was accorded to Mr. Thackeray for his paper.

The following papers were announced:—

S. W. Burnham. "Sirius."

John Tebbutt. "Observations of Phenomena of Jupiter's Satellites at Windsor, New South Wales, in the year 1890."

A. Stanley Williams. "Reduction of Measures of the Photographs of Jupiter taken at the Lick Observatory in 1890."

Col. Bassot and *Commandant Defforges.* "Sur la détermination de la longitude Paris—Greenwich."

H. H. Turner. "On the recent Determination of the Longitude of Paris—Greenwich. Reply to Col. Bassot and Commandant Defforges."

W. G. Thackeray. "A Comparison of the North Polar Distance of the 'Nautical Almanac' for 1880 with the Cape Catalogue, the Greenwich Ten-year Catalogue, and Boss' Standard Star-places for 1880."

W. G. Thackeray. "Approximate Proper Motions of some Groombridge Stars."

L. Becker. "On the Orbit of the Periodic Comet 1867 I."

S. W. Burnham. "Invisible Double Stars."

R. Bryant. "The Perturbations of Sappho 80."

A. Marth. "Ephemeris for Physical Observations of the Moon, 1891."

John Billington Booth, J.P., Preston, Lancashire, and *Hannibal Mirabita*, 134 Strada Britannica, Alexandria, were duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Richard Edward Synge Cooper, Gentleman, Hillmorton Paddox, Rugby (proposed by J. C. Adams); *H. F. Newall*, Trinity College, Cambridge (proposed by H. H. Turner); and *Samuel H. Ridge, B.A.*, 275 Victoria Parade East, Melbourne, Australia (proposed by P. Edward Dove).

MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

Wednesday, April 15.

MR. A. BREWIN, *Vice-President*, in the Chair.

MR. J. BAXENDELL, Mr. H. Champ, and Mr. S. H. Ridge, B.A., F.R.G.S., were elected Fellows.

The following papers were read:—

1. "Some Remarkable Features in the Winter of 1890–91," by Mr. F. J. Brodie. The author points out the peculiarities or special features of interest in the weather which prevailed over the British Isles during the past winter. In addition to the prolonged frost, which lasted from the close of November to about January 22, he finds that the barometric pressure for the whole winter was about a quarter of an inch above the average; and that when the wind was not absolutely calm there was an undue prevalence of breezes from some cold quarter. The percentage of winds from the southward did not amount to one half of the average. The number of foggy days in London was no less than twice the average. The rainfall over the greater part of the British Isles was less than half the average. The author says that almost every element in the weather has been influenced to an abnormal degree by the remarkable prevalence of high barometrical pressure, and if we were called upon to define the season 1890–91, we should have little hesitation in giving it the name of the "anticyclonic" winter.

2. "The Rainfall of February 1891," by Mr. H. S. Wallis. This was one of the driest months upon record; the mean rainfall over England, excluding the Lake District, being only 0·066 inch, or about one fortieth of the average.

3. "On the Variations of the Rainfall at Cherra Poonjee in the Khasi Hills, Assam," by Mr. H. F. Blanford, F.R.S. Cherra Poonjee has long been notorious as having a heavier rainfall than any other known place on the globe, the mean annual fall being frequently given as about 600 inches. Mr. Blanford has made a critical examination of the various records of rainfall kept at this place, and has come to the conclusion that the above amount is too high, and that the average annual rainfall is probably only a little over 500 inches.

MEETING OF THE ASTRONOMICAL SOCIETY OF THE PACIFIC.

1891, March 28.

The Directors met prior to the regular meeting and elected 2 life-members and 21 ordinary members.

The Annual Meeting was held by invitation in the Lecture-hall of the California Academy of Sciences immediately after the Directors' Meeting.

Mr. Holden in the chair.

The minutes of the last meeting were approved.

The Secretary read a list of seventeen presents received, and the thanks of the Society were voted to the givers.

The list of new members elected was read to the meeting.

The Committee on Nominations reported a list of names proposed for election, as follows:—

For Directors: Messrs. Alvord, Burckhalter, Hill, Hilborn, Holden, Keeler, Molera, Pierson, Schaeberle, Soulé, Ziel.

For Committee on Publication: Messrs. Holden, Keeler, Yale.

The cordial thanks of the Society were returned to the California Academy of Science for their invitation to hold its meetings of March and November 1891 and January 1892 in the Lecture-hall of the Academy building, and the invitation was accepted.

Messrs. McConnell and Veeder were appointed as tellers. The polls were open from 8.15 to 9 P.M., and the persons above named were duly elected.

The Treasurer then read his annual report, as follows:—

Receipts:		
Cash balance, March 29, 1890		$1,121 75
Received from dues	$1,957 13	
Received from Publications	23 50	1,980 63
		$3,102 38
Expenditures:		
For Publications and general expenses	$2,347 89	
On account of Montgomery Library Fund	9 84	$2,357 73
Cash balance in Bank, March 28, 1891		744 65
		$3,102 38
Funds:		
Alexander Montgomery Library Fund, cash balance		$1,590 11
Donohoe Comet Medal Fund, cash balance		527 35
Cash in bank (General Fund)		744 65
Total Cash on hand		$2,862 11

The Committee appointed to audit the Treasurer's accounts reported the Treasurer's accounts as correct, and the report was accepted and adopted and the Committee discharged.

The Committee on the Library reported that the library consists, at present, of 491 volumes, 431 of which belong to the Alexander Montgomery Library, and were purchased with the Alexander Mongomery Fund, and 60 volumes have been derived from other sources, by contribution and purchase.

The cost of the Alexander Montgomery Library to date has been $1,017.24.

The Report of the Committee on the Comet Medal was as follows:—

The Comets of 1890 have been:

Comet *a*, discovered by W. R. Brooks, Geneva, New York, March 19.

Comet *b*, discovered by J. Coggia, at Marseilles, July 18.

Comet *c*, discovered by W. F. Denning, at Bristol, July 23.

Comet *d* (D'Arrest's periodic comet), re-discovered by E. E. Barnard, at Mt. Hamilton, October 6.

Comet *e*, discovered by T. Zona, at Palermo, November 15.

Comet *f*, discovered by R. Spitaler, at Vienna, November 16.

Medals have already been delivered to Messrs. Brooks, Coggia, Denning, Spitaler, and Zona.

In November last, Mr. Barnard addressed a letter to the Committee giving reasons why the medal should not be awarded for the discovery of periodic comets, especially as recent observations have made it at least possible that the brighter periodic comets may be followed throughout their whole orbits, and he signified his desire that the medal should not be awarded to him for the rediscovery of D'Arrest's comet, although the Committee was prepared to make the award. In some other respects also the rules were found to be cumbrous. They have accordingly been thoroughly revised and submitted to the founder, who has signified his acceptance of the changes, and in their revised form they were adopted by the Board of Directors on February 26, 1891, and take effect from that date. Copies of the new rules have already been sent to the principal astronomical journals and to all our corresponding observatories and institutions.

A Committee of seven members was appointed to make arrangements for an astronomical exhibit at the Columbian Exposition in Chicago.

The following papers were announced:—

a. "The Fireball in Raphael's Madonna di Foligno," by Prof. H. A. Newton.

b. "On the Similarity of Certain Orbits in the Zone of Asteroids (2nd paper)," by Prof. Kirkwood.

c. "Astronomical Observations in 1890," by Torvald Kohl, Denmark.

d. "Address of Retiring President of the Society," by Prof. Holden.

e. "A Few Hints to Beginners in Solar Observations," by Miss E. Brown.

f. "Lunar work for Amateurs," by Thomas Gwyn Elger, F.R.A.S.

g. "The Total Solar Eclipse of January, 1889," by Prof. H. S. Pritchett.

The President stated that the Society would have the pleasure of listening to some remarks from the Earl of Rosse, who is now visiting this city, and who occupied a seat on the platform with him.

Lord Rosse selected for the subject of his brief talk to the members the "Reflecting Telescope," of which class of instruments the mammoth speculum at Birr Castle is at once the largest and the most famous. He expressed the hope that a good-sized reflector may at some time be added to the Lick Observatory equipment, in order that the merits of the two classes might be favourably compared. He gave an interesting account of the manufacture of the "great reflector" which his father constructed through the aid of country mechanicians, entirely unskilled in delicate workmanship. The mirror was six feet in diameter, and the supporting tube so large that the tallest man could walk its entire length. This instrument revealed new features in all the nebulæ, which were selected for its special field of work, since all the 2000 objects in Herschel's catalogue were again examined. The views afforded of the nebular construction have been unexcelled to this day, but entire refitting of the mechanical parts would be necessary to meet the requirements of modern research, in order to relieve strain and keep unchanged the symmetrical figure of the speculum.

Lord Rosse gave some account of his researches in determining the amount of heat radiated by the Moon, a quantity which the observers had failed to detect by means of refractors, for the reason, as he explained, that the lenses absorbed over four-fifths of the heat-rays. In the line of astronomical photography, also, the reflecting telescope, as is well known, possesses the great advantage of bringing all the spectroscopic colours to the same focus.

The renowned Parsonstown astronomer was warmly received and his modest story listened to with interest.

The President read his annual address, showing that the Society starting with 40 members 2 years ago, now had a membership of 381 life and active members. Nearly half the members were non-resident, the Society being represented in 26 States of the Union and 19 foreign countries.

After the regular meeting, the new Board of Directors met and elected the following officers for the ensuing year:—

President: W. M. Pierson.

Vice-Presidents: Frank Soulé, J. M. Schaeberle, E. J. Molera.

Secretaries: Chas. Burckhalter, James E. Keeler.

Treasurer: F. R. Ziel.

Committee on Publication: Messrs. Holden, Keeler, Yale.

CHAS. BURCKHALTER, *Secretary*.

The Astrophotographic Chart.

Those who were present at the recent meeting of the Permanent Committee for the execution of the Astrophotographic Chart, or who have had an opportunity of reading the 'Procès-verbaux'[*], could not but be struck with the great earnestness of all concerned. The members assembled in Paris last Easter were of many different nationalities; of different astronomical departments and reputations; and, as the discussions plainly showed, held very different views on several points of detail, and even on the general scope of the work. But one feature was common to all—a deep-felt anxiety that the best course should be chosen, and a determination that no effort should be spared to secure this selection.

Whatever may be the result of the united endeavour to chart the Heavens as they are at the end of the nineteenth century, the scheme cannot be called ill-considered; for men in the front rank of almost every department of Astronomy have contributed to the discussions, and occasionally pledged their professional reputations in favour of some amendment where their experience was at variance with the course proposed. So enthusiastic were the dissentients, and so unscientifically heated did they become, that they were on two occasions fain to adjourn the discussion in order to arrive at some compromise in a calmer atmosphere; and while one cannot but wonder that there should have been such differences of opinion on questions so nearly allied to matters of fact, one must at the same time recognize the keen interest taken in the project; and those of hopeful temperament will probably feel that so much energy must be richly productive.

Over the actual decisions of the Committee, where they touched matters of principle, we cannot profess to rejoice. On two broad questions we should have taken the side, or rather we have already taken the side, which was only able to secure a compromise after a rather sharp conflict. We consider that the scope of the work has been illegally magnified, and a catalogue illegally substituted for a chart; and the meaning of the word illegal is that the decisions of the Committee on these two points are, in our opinion, contrary to those of the real legislative body—the Congress of 1887. The question of a Catalogue is one on which we have already spoken with some warmth and at some length; and we cannot accept the decisions of the Committee as evidence that we misinterpreted the views of the Congress—simply because we still believe the Congress would condemn those decisions. The Committee is largely composed of those who have hitherto been professionally engaged in making Catalogues, and who may thus naturally have a prejudiced view of their importance, while the Congress gained in breadth of view by the association of men from other departments of Astronomy, and even other branches of science. On the other hand, it is curious that the Committee of experts should be the body to enlarge the scope of the work; and that practical astronomers should draw from a physicist (for we may so style M. Cornu without in any way disparaging his great services to Astronomy) the remark—"Ce qui me préoccupe surtout c'est de voir la tendance qui se manifeste dans la Conférence à vouloir toujours augmenter un travail déjà si considérable en lui-même" (Procès-verb. p. 51). We would respectfully associate ourselves with M. Cornu in his anxiety. We feel the same trepidation as would probably thrill the friends of a chessplayer who was to meet an antagonist of unknown powers, and who insisted on playing blindfold; or of a strong man who was to lift a weight of unknown specific gravity, and who himself revised the programme by quadrupling the weight. If anyone thinks these metaphors exaggerated, let him compare the present programme with that of the Congress of 1887. It was then determined to make a chart of the Heavens by taking photographs of 15 to 20 minutes exposure. By successive interpretations and additions this has now come to mean photographing the heavens six, possibly eight, times over, with a total exposure of about 90 minutes, or perhaps 110 for any one star. The figures are necessarily provisional owing to recent decisions of the Committee; but we are certainly underestimating the desires of several members in reckoning 2 exposures of 2 minutes and 4 respectively for each series of Catalogue plates; 40 minutes for the first Chart series (as already decided); and at least another 40 minutes for the second, if not three exposures of 20 minutes each. And this is only the compromise,—the concession wrung with some hard fighting from the more ardent spirits! It is difficult to say what proportions the undertaking would by this time have assumed if some members of the Committee had not been duly impressed with the danger of attempting too much. To these we feel deeply grateful; and we hope that their determination in discharging a somewhat ungracious duty may be rewarded by a more authoritative tribute of approval. Their task was not an easy one: they were tempted by proposals from the opposition, speciously disguised so as to look very like their own. Would it not be better, they were asked, to start with a simple programme and take the Catalogue plates first? The simple programme is an attractive bait; but fortunately there were those who were quite determined that to make a Catalogue in the present and leave the Chart to a more or less indefinite future was precisely what they were *not* going to do. We greatly regret that here again numbers were so far against them that they were only able to obtain a compromise, and were fain to be content with the following resolution:—"Les travaux décidés par le Congrès de 1887, comprenant deux séries de clichés faites avec des poses différentes, le Comité permanent tout en recommendant aux observateurs de pousser avec la plus grand activité possible l'exécution des clichés de la deuxième catégorie (clichés destinés à la construction d'un Catalogue) est d'avis qu'on devra aussi profiter du plus grand nombre possible de belles soirées pour faire des clichés à longue pose de la première série."

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[*] For a general account of the Meeting see the remarks of the Astronomer Royal on p. 172.

So the Catalogue is to be pushed forward and the poor Chart only tolerated; and the primary exposure for the Catalogue plates is defined as twice the length of the secondary, instead of the secondary being defined as a quarter that of the primary; and the time of exposure for the Chart should apparently, in the opinion of some members of the Committee, be decided by the requisites of its paper copies; and subsidiary things generally are to be put into the important place.

It is bewildering, and not a little distressing. The difference between a Chart, an autographic representation of the stars as they exist at a certain time, forming a groundwork for investigation, any portion of which may be selected for a special research, and measured with an accuracy suitable for the particular purpose, or compared with the heavens or a similar representation at a future time; and a Catalogue, an investigation in itself, which already bears the impress of selection in the number or magnitude of stars recorded, or in the precautions taken in measuring them, and moreover of an indiscriminate selection in that beyond a certain point all stars are treated similarly—the difference between these two things is, to our minds, so clear that we are puzzled and distressed when we find the latter being shuffled into the place properly belonging to the former.

And here we are speaking of a Catalogue such as is advocated by more than one person, as an end in itself—more important, or at least of greater urgency, than the Chart. The Catalogue of Reference Stars voted by the Congress of 1887 was a different thing, merely a collection of pegs to peg down the separate leaves of the Chart in their proper place, whose urgency was, therefore, strictly determined by that of the Chart, to which it was subsidiary. Admiral Mouchez, in his opening address, now speaks of "le résultat utile et pratique d'un Catalogue jusqu'à la 11^{e} grandeur auquel beaucoup d'astronomes attachent la plus haute importance," and says of the Chart, "elle n'a pas la même urgence que le Catalogue" (Proc.-verb. p. 6). Had he kept in view the original reason for the Catalogue it is difficult to understand how he could have advocated determining the places of the "Etoiles de repère" at a different epoch from that of the Chart, of which they are to be the fundamental points. It is too much like making the fittings before the house is built, because it is of such importance that they shall fit nicely. But once the importance of the Catalogue in itself is admitted, Admiral Mouchez is of course quite right in urging its execution; for the photography is relatively easy, and the measurement and reduction will take a long time. We can only regret that this view is advocated so strongly. In our view such a Catalogue is a mere human reproduction of a

Chart, and stands condemned in the words of M. Cornu (Proc.-verb. p. 44) about other projects for reproduction: "Nous n'avons le droit de faire usage d'aucune méthode qui ne reposerait pas sur l'emploi de la lumière, et la lumière doit agir seule,—il ne doit y avoir aucune intervention de la main humaine. Cela serait contraire au caractère de notre entreprise."

To return for a moment to the question of exposure. The most remarkable occurrence of the meeting was perhaps the extraordinary paper read by Dr. Scheiner on the connection of magnitude and exposure. At the eleventh hour he gravely announced that one of the fundamental principles of the work was quite wrong, and that an exposure of 2½ times the duration was only equivalent to about a half instead of a whole magnitude. We are very sorry for Dr. Scheiner; he no doubt believed in his results, and bravely undertook the duty of giving the proper warning. His reputation and the admirable way in which he stated his case gave his words a weight which they could not otherwise have acquired, and made an obvious impression on the Committee which had some unfortunate results. For we need hardly say that we cannot accept his facts. There must be some mistake, of what kind it is impossible to say with certainty; but we may surmise that the weighty words of Capt. Abney may be applicable to this case, viz.: "beaucoup de plaques examinées jusqu'à présent ont été plus ou moins voilées par l'action de la lumière diffuse, et ce fait est une des causes qui expliquent qu'on ait trouvé des formules différentes pour les grandeurs" (Proc.-verb. p. 23). The evidence against Dr. Scheiner is overwhelming. It was already so strong at the time of the 1887 Congress that the Brothers Henry referred to it in general terms, without considering it necessary to give details.

In the 'Procès-verbaux,' p. 38, we read:—"M. Paul Henry fait connaitre le résultat obtenu sous ce rapport (sur la durée de pose nécessaire, etc.) en se servant de l'équatorial de $0^{m}\cdot33$ d'ouverture.

"Il faut en moyenne une durée d'environ quinze minutes pour obtenir à peu près les étoiles de 14^e^ grandeur.

"On sait d'ailleurs que, pour passer d'une grandeur à la suivante, on doit multiplier le temps de pose par 2·5 environ."

On these facts the resolutions of the Congress were based; and since that time evidence in confirmation of them has accumulated, while little or nothing has been authoritatively urged on the other side. That Dr. Scheiner was heard with such respect, and his words allowed to influence the resolutions as far as they did, is probably the greatest compliment the Potsdam Observatory will ever receive. As a result of his remarks the first exposure for the Chart plates is to be 40 minutes instead of the 15 suggested by M. Paul Henry in 1887; and some otherwise interesting but not immediately pressing experiments with screens are to be added to the programme, which will delay the commencement of the work several months.

In conclusion, we are glad to turn to some of the thoroughly satisfactory features of the meeting. Admiral Mouchez had the pleasure of reporting to the Committee several substantial acts of liberality and generosity. The promptitude with which M. Bischoffsheim responded to the appeal for funds to make a parallactic micrometer has been already recorded with gratitude; and at this, the final meeting of the Committee before entering on the work of execution, the President rightly recalled to its memory the graceful action by which the French Academy undertook the publication of the 'Procès-verbaux' and 'Bulletin.' It is very pleasant to hear that M. Biver, Director of the Saint-Gobain Cristallérie, not only guarantees perfect plate-glass for the negatives, but offers them for the simple cost of production; and that the house of Lumière in Lyons similarly offers not to make a commercial question of the conversion into photographic plates. We may perhaps add an instance of a slightly different character from the report of Father Denza on the Vatican Observatory, founded principally for celestial photography. He says (Proc.-verb. p. 11):—"Nous sommes autorisés par le Saint Père à faire tout ce qui sera possible et nécessaire pour mener à bonne fin l'entreprise."

This shows a spirit of determination to get the best possible result, which we have already remarked is characteristic, and we sincerely congratulate Admiral Mouchez and all the original promotors of the scheme on the great enthusiasm they have evoked, of which even the opposition he has received from ourselves and from others of greater authority and importance is, if rightly interpreted, only one phase.

Any notice of this meeting would be incomplete without a reference to the services of two gentlemen whose names we were glad to see added by acclamation to the Committee on Reproduction—M. Cornu and Capt. Abney. No one can read the 'Procès-verbaux' without being struck with the importance and efficacy of their remarks. We have already quoted M. Cornu twice, and we might have added other words of wisdom with advantage. Capt. Abney placed his experience as a photographer unreservedly at the disposal of the Committee, who have to thank him for guidance on more than one difficult question. EDITORS.

Notes on Visits to some American Observatories.—II.

[Continued from p. 156.]

WE found Washington in a state of Christmas festivity; the snowstorm, which hurried us out of Virginia, had sprinkled enough snow on the streets and surrounding roads to bring out all the sleighs in the neighbourhood, and it was as much as one's life was worth to attempt to cross Pennsylvania Avenue on foot in the afternoon or evening. Pleasure and swiftness in sleighing seem to be connected by a logarithmic relation, and the magnitude of an additive constant is such as to lead to remarkable ingenuity in the device of sleighs. Some interesting questions in stability have thereby been solved practically: it gives one, for instance, new ideas of a rocking-chair to see it gliding with lightning speed after a horse.

The surroundings of the Washington Observatory are in very marked contrast to those of the Lick. At Mount Hamilton, however great the actual achievements in work are, the predominant feeling that must arise in the mind of a visitor accustomed to ordinary sites is one of elation and enthusiasm in the thought of the *opportunities*. At Washington, there can be no doubt, one is primarily impressed with the admiration for the *work* achieved under what may outspokenly be described as unfavourable conditions. The Potomac is unkind to the astronomers, and all look forward to the removal to the fine site on high ground above Georgetown. There the instruments will be above the lower lying mists which now cause trouble in observing, and the ardour of the workers will not be deadened by suspicions of malarious sickness.

Prof. Hall kindly devoted considerable time to showing me all details of the Observatory, and especially the 26″, and allowed me to spend part of a wonderfully good observing evening with him looking through the 26″. There is that certain air of homeliness about the general appointments of the 26″ that betokens solid working. The sail-cloth that serves as shutter to the dome, the simple observing-chair, the well-worn look of the micrometer always attached to the telescope, the general appearance of the clock in the stone pillar supporting the telescope—everything points to an evident intention to get immediate and constant returns in results. The mounting itself cannot fail to strike one as astonishingly light; it is, I suppose, as a matter of fact, too light. The counterpoise, intended to relieve the declination axis of strain, can easily be set in vibration in such a way as must cause inconvenience, if nothing worse, in observation. It takes a very appreciable time for the instrument to come to rest after being shaken.

Prof. Hall is busily completing his observations on double stars for his catalogue, and continues his observations of satellites.

Prof. Harkness is engaged upon a gigantic piece of computation, and has nearly completed it. He has proposed to himself to attack the problem of deducing from the enormous accumulation of observations, which have been made by various observers for special ends, values of certain constants connected with the determination of the solar parallax. What one may call fundamental constants in the solar system appear in every astronomical determination, some of them in their own right, so to speak, others by implication in quantities got by observation. Prof. Harkness's wish is, to extract from all such observations as are to hand values for the constants implied in them, and so to get values which represent really our present knowledge. It is a colossal problem to undertake; but so much way has been made that part of the calculations are in print.

Prof. Paul is continuing observations on the shortest-period variable known (S Antliæ, period 7 hours odd).

The grounds of the new site cover 70 acres, and have a circular boundary. The buildings are to be ready by this summer; but it is expected that the move can hardly be complete before next spring. Congress was to be asked to appropriate $295,000, required in abandoning the old and equipping the new Observatory. Many new instruments are contemplated, notably a 13″ photographic telescope, a 7″ heliometer (the only other heliometer in the States is that at Yale), a 6″ transit-circle, and provision is made for a 12″ equatorial. The 26″ will have a new mounting by Warner and Swasey, of Cleveland, who are doing so much of this kind of work. It is proposed to have a movable floor of Grubb's device: it seems questionable whether this is worth the trouble and expense in a 40-foot dome, unless the 26″ is to be used much in photographic and spectroscopic work. The new dome is to be made of steel, mounted on stone upright walls; it interested me much to learn that everyone regrets that the upright wall, which is already built, is not to be of steel. Two considerations led my father to choose iron as the material for both the walls and the dome now being set up at Cambridge—one was portability, the other was equalization of temperatures. At Washington the evident feeling is that the latter is a most important one, and is realized best by steel building. And as bearing on the former, it is of interest to note that the peripatetic Observatory emanating from Harvard College, and now under the direction of Prof. W. H. Pickering in Peru, is provided with buildings made with wood and covered with paper—an arrangement which has been found very satisfactory for such work.

On the pleasant memories of two days spent at Baltimore after leaving Washington on New Year's eve, I must not here dilate, even to tell of a visit paid to Prof. Rowland. The machine for ruling diffraction gratings compels reverence as the most peaceful machine ever made, not to mention any of its physical excellences. From Baltimore we went on to Philadelphia, and spent a Sunday there visiting friends, and thence to Boston, passing by night through New York. The whole train was transferred on a ferry boat across the Hudson, which was filled with blocks of ice and snow: these seemed to cause wonderfully little inconvenience to the ferry. Thus we passed up the East River, with a most impressive scene around us. Brooklyn Bridge overhead, with the rising curve sketched out by the lamps upon it and by the lighted cable-cars creeping along backwards and forwards, and the dark silhouette of the city on the left, looming mysteriously against the sky, without a hint of the unsightliness of many of the tall buildings with bare sidewalls waiting for neighbours to rear themselves beside them.

At Boston we spent ten days, and met with kindness on all sides that we are not likely to forget. Of the many points of interest which Prof. Pickering had to show, I must needs pick out only one or two. The general feature of the Observatory of Harvard College which distinguishes it in the mind of the visitor, is perfect organization to attain certain specified ends on an immense scale. One has a sense of there being a great army there, marching on, conquering vast tracts; now and then a cohort may be told off to rest upon some special spot and there to gather their spoil, and then follow on, ready to join in the general advance. The commander-in-chief himself gathers might from might, and realizes without consternation that his army is dealing with extension in three dimensions, the field for investigation is increasing with the square, and the mass of information to be collected with the cube of the radial extension. One gets some idea of the activity at Harvard, when one learns that 9000 photographs have come into the Observatory during the last year. In his last report, Prof. Pickering points out in solemn terms the danger to which the plates, of which there are now more than 27,000 in the Observatory, are exposed from the possibility both of fire and of the giving way of floors, which were not specially constructed to carry heavy loads, under the weight of the plates.

The photographic doublet is much favoured at Harvard on account of the extent of field. The 8-inch Bache telescope is of this nature; it is provided in front with large prisms of aperture not less than that of the objective. It is with this arrangement that the general material for the Draper Catalogue of Stellar Spectra has been collected, and so serviceable has it proved that when the Bache telescope was sent away to collect further material in Peru, Mrs. Draper decided to provide a duplicate, and this is now in constant use at Harvard. It was a very great pleasure to meet Mrs. Draper at Harvard, and again to enjoy her friendly hospitality on the eve of our departure from New York.

Each plate taken with these instruments includes a region of 10 degrees square, and often shows as many as 200 spectra distinct enough to be measured. A 24″ doublet with prism in front is to be made to extend the work to fainter spectra on the wholesale method. Miss Bruce of New York, of whose keen interest in astronomical progress so many astronomers all over the world have lately such lasting proof, is the donor. Messrs. Clark have already received the glass for the prism, and a very perfect piece of glass it appears, as it lies in their workshop, about 3 inches thick, and almost completely free from blemish. The discs for the doublet have not yet arrived.

The Draper Catalogue of Stellar Spectra is already printed and forms volume xxvii. of the Annals of the Harvard College Observatory. It is a volume of 388 pages, of which 105 pages are devoted to the actual Draper Catalogue, the rest containing details of measurement and notes of further information which has not been condensible into a single letter. For any one star, the entry consists of half a line in a quarto page. Three columns give the reference numbers in the Draper Catalogue (D.C.), the Durchmusterung (D.M.), and the Harvard Photometry (H.P.), the numerical order in the D.C. being decided by R.A. in 1900; two columns give R.A. and Declination 1900. Another column gives the number of plates on which measurements have been made for that particular spectrum. The sixth column contains a letter designating the class of spectrum; the seventh gives the mean of the photographic magnitudes, and the last column gives difference between this mean and those in the D.M., the Argentine General Catalogue, and in the Harvard Photometry, the quantity measured in each case in the D.C. being the intensity of the spectrum about the *g* line.

The ordinary classification was too broad to meet all cases, and hence the spectra have been classed under letters A, B . . . Q arbitrarily assigned, and these arbitrary classes may easily be translated into any system. Photographs of typical spectra are given in the frontispiece.

A second table of details of measurement follows the catalogue; in it each line of entry contains (i) the D.C. number of the star, (ii) the reference number of the actual plates on which the spectrum appears, (iii) the observed brightness on each plate, (iv) residuals, (v) the end of the blue designated by Fraunhofer or special letters, (vi) a letter describing the nature of further information to be found in remarks contained in about 100 pages at the end of the volume.

Table III. is a catalogue of photometric residuals exceeding ·09.

Table IV. is an index to letters employed by Bayer.

Table V. is a summary of stars in the Draper Catalogue.

Of volume xxvi. of the Annals only part is printed; when completed it will form a volume of which the Draper Catalogue volume may be regarded as the tables, and will contain a description of the formation of the tables and of the work done with the Bache telescope, and a discussion of that work.

Photographic charts showing all stars to the 12th magnitude have been obtained, and thus it is possible at once to verify variability, which subject has become one of great interest in connection with the spectrum work, inasmuch as "the hydrogen lines are shown to be bright in thirty variable stars of long period," which "peculiarity of the variable stars furnishes a means of discovering them."

H. F. Newall.

[To be continued.]

The 'Capture' of Periodic Comets[*].

The orbits of the short-period comets present so many points of resemblance that it has been suggested, on grounds of more or less probability, that some common origin, or at least some common cause, has operated to give to the orbits those special features by which they are characterized. These features include direct motion in the orbit, which is but little inclined to the plane of the ecliptic; the coincidence of the aphelion distance with the mean distance of Jupiter, and at one of the nodes there has usually been at no very distant date a close approach to the planet itself. It is very easy to suggest from these facts that the action of Jupiter has converted an orbit which was originally of a parabolic type into one that is elliptic. Such an obvious suggestion, however, is not without its difficulties. If we suppose a comet, moving with the velocity necessary to produce parabolic motion, to approach a disturbing planet, one of two things must happen: either the velocity is diminished, in which case an ellipse results (and this is the only condition with which our experience has made us familiar), or the velocity is increased, and the comet moves along a branch of a hyperbola. Since either event is *primâ facie* equally probable, why is it that we have no instance of a hyperbolic comet approaching the Sun, after having been in collision with the sphere and activity of Jupiter? Apparently we ought to have seen as many instances of hyperbolic as of elliptic motion.

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[*] Etude sur la théorie des comètes périodiques, par M. O. Callandreau.

There is, too, another difficulty for which it has not been easy to offer an adequate explanation. This difficulty arises from the comparatively small area of the sphere over which Jupiter may be considered to have sway. The radius (ρ) of this sphere is for any planet

$$\rho = r\sqrt[5]{\tfrac{1}{2}m^2},$$

where r and m are respectively the radius-vector and mass of the planet. In the case of Jupiter, ρ is approximately $\frac{1}{20}$. But the theoretical probability that a comet encounters this sphere is of the order ρ^2 or $\frac{1}{400}$; and therefore of 400 comets which have passed the orbit of Jupiter, one only should exhibit elliptic motion from this cause. But there are some twenty comets which have been proved to move in ellipses of short period.

Recently a very thorough examination of the problems arising from the approach of a comet to Jupiter has been undertaken by a most competent authority. In the 'Bulletin Astronomique,' vol. vi., M. Tisserand has indicated a method of dealing with the question, and has deduced some interesting results, more particularly depending upon the eccentricity and mean distance. This problem has been still further elaborated by M. Callandreau, who, besides reproducing much of M. Tisserand's work in an extended form, takes into consideration the other elements of the orbit, and in some particulars gives us the means of comparing the theory with observed facts. M. Callandreau has shown the conditions under which an ellipse with direct motion may result from contact with the sphere of activity of Jupiter, and it appears that the character and direction of the motion of the comet, after passing away from the neighbourhood of Jupiter, depend on the angle at which the comet entered that sphere. For instance, if, following M. Callandreau, we describe as the *apex* that point at which the tangent to the orbit of Jupiter meets that sphere, measured in the direction of the planet's motion, then elliptic orbits with direct motion result from a parabolic comet encountering the sphere near that point. On the other hand, hyperbolas with direct motion are produced by the original parabolic comet entering on the opposite side or *antiapex* of the sphere, while ellipses with retrograde, and hyperbolas with direct motion follow from contact with the sphere at angles 90° distant from either of these points. M. Callandreau, availing himself of the 'Catalogue of Cometary Orbits,' drawn up by M. Schulhof, compares his theory with observation, and he finds that, with the single exception of Encke's comet, the twenty known orbits of short period can be explained by supposing them to be produced by parabolic comets moving towards the apex, and having a perihelion distance comprised within the limits of r and $\frac{r}{2}$.

The anomaly presented by the case of Encke's comet can be explained by supposing an approach to one of the planets inferior to Jupiter and undergoing additional perturbation*. The part played by the inferior planets in modifying the motion of comets may be considerable and yet avoid detection. M. Callandreau favours the plausible hypothesis, and one that does not contradict the latest spectroscopic investigation, by supposing these planets to act in their respective spheres of activity in the same way that Jupiter acts, though within an area more restricted on account of the smallness of their masses. They would thus give rise to a numerous class of elliptic comets of very short period, which could furnish not only an explanation of the zodiacal light, but would supply a medium of a certain resistance, which the observed irregularities in the motion of Encke's comet appear to demand.

A further test to which the capture theory may be submitted is the determination of the limits between which the eccentricity of an orbit of a comet having a given time of revolution must lie. This test cannot, however, be applied rigorously, but the result of its application is as follows:—Taking the sixteen comets whose inclinations are less than fourteen degrees, it is found that eight satisfy the test, having eccentricities within the computed limits, while the remainder, though discordant, do not pronounce decisively against the theory. The uncertainty in the criterion arises from the fact that we may have to deal with an ellipse of long period, reduced by perturbation to a shorter, whereas the theory in its rigour deals simply with a parabola. And here it should be remarked that the theory does not require that the capture should be effected at a single approach. It is shown that the action of Jupiter can gradually prepare the way for final capture by diminution of the period at successive returns. The effect of the uncertainty of this criterion on the computed eccentricity of an orbit, when the axis major is not infinite, is illustrated by Brooks's Comet of 1889, which with a mean distance of 3·67 ought to have an eccentricity greater than 0·54, supposing the change from parabolic motion to have been effected at one effort. The observed eccentricity is, however, 0·47; but such a value would have resulted if Jupiter perturbed, not a parabola, but an ellipse, whose semi-axis major was about 100, that is whose period was about 1000 years. It is true that, judging from some modern instances, the life of a periodic comet cannot be indefinitely extended, and therefore the long periods of time necessary to produce encounters between Jupiter and the comet seem to tell against the hypothesis. But it is not probable that, remote from Solar activity, there is the same tendency to disintegration or disappearance that has been observed in such comets as Biela or De Vico.

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* In a conversation which the writer recently had with M. Callandreau he suggested that the introduction of Encke's comet into the Solar system was due to the action of Mercury. Though M. Callandreau declined to express any definite opinion, and treated the question with the greatest reserve, he was understood to distinctly favour the suggestion. A further proposal that Halley's comet might have received its present form from perturbations of the Earth was not received with the same favour.

The discussion of the perihelion distance and of the position of the line of apsides yields still further support to the capture hypothesis. A satisfactory explanation is afforded of the observed coincidences of the aphelion of the comet with the orbit of Jupiter, by the demonstration of the fact that the line of apsides makes an acute angle with the radius-vector drawn through the point of closest approach. This law obtains throughout the table of orbits referred to, while in thirteen out of twenty cases the coincidence is almost exact. With regard to the perihelion distance, since in the original parabolic orbit the least distance varied from r to $\frac{r}{2}$, *i.e.* from 2·60 to 5·20, it is evident that a very considerable diminution must have resulted from perturbation, and M. Callandreau shows that in the case of comets with *direct* motion the perihelion distances are necessarily diminished, and thus brought within the range of observation; while comets with retrograde motion are removed still further from the Sun, and therefore necessarily escape detection.

The fact that the influence of Jupiter has not produced instances of hyperbolic motion is also examined analytically with a satisfactory result. The conclusion is that hyperbolic comets elude observation, either because the perihelion distance is large, beyond the orbit of Mars, or because a comet once captured does not approach perihelion, but travels along the hyperbola to an infinite distance.

The effect of the investigation of MM. Tisserand and Callandreau, conducted with consummate ability, has been to remove much that was vague and unsatisfactory, and to bring the capture theory within the range of great probability. W. E. Plummer.

Variations in Nebulæ.

IN observations of nebulæ great differences are induced by atmospheric variations. Last autumn I frequently looked at a small, round nebulosity which I had discovered in R.A. $8^h\ 34^m$, Dec. 85° 54′ N. On certain nights it was quite plain with a power of 60 on my 10-in. reflector, on others I could barely glimpse the object. I felt inclined to believe that real variations occurred in the inherent light of the nebula, but further observation proved the changes were due to the varying transparency of the atmosphere. Another small nebula with a nucleus was examined on different evenings, and it is remarkable that on some nights I recorded it as having a stellar nucleus, while on others it appeared to have an ill-defined central condensation. Its brightness apparently fluctuated as well. But I ascertained that these alterations were also due to the varying condition of the air. Bad definition gave the nucleus a woolly, diluted appearance, and the visibility of the nebula varied with the opacity of the atmosphere.

Fog, haze, smoke, and wind exercise a most perceptible influence on faint, delicate objects. When planetary definition is good the details of a nebula are usually distinct if the sky is dark and no fog or thin cloud is near. There is a prevalent idea that large aperture rather than good definition is required for small comets and nebulæ, but for the perception of minute features the latter quality is most important. Comparatively small telescopes of high excellence will do wonders (as D'Arrest showed) on nebulæ, and will exhibit detail and faint objects that are ordinarily considered beyond their reach. With a small aperture the field of view is relatively darker than in a large one, and the disparity of size certainly does not give the larger instrument a commensurate superiority of power.

It has been alleged that the nucleus of the great nebula in Andromeda undergoes some striking variations in its physical character. These variations have been noted by means of photography as well as by the direct evidence of the telescope. Apparent changes undoubtedly occur, but it is by no means certain that actual changes are constantly affecting the nucleus. The variations appear to be of a fugitive, alternating character, and are very likely to have their sole origin in atmospheric disturbances. An observer who often examines this fine object will be sure to remark differences in its appearance; but when he records the varying state of the atmosphere, as affecting definition, as well, he will pretty certainly find that the former are closely associated with the latter.

Descriptions of nebulæ by various observers are seldom in harmony, because the seeing at the times of their several observations was not equally good. A pretty bright nebula seen in a bad sky is liable to be considered faint. There is a nebula (N.G.C.

6015) 4° W. of η Draconis which Sir W. Herschel described as "very faint," while D'Arrest called it "bright." N.G.C. 1469, near γ Camelopardi, was discovered by Swift and described as "very faint;" but I picked it up on a very clear night in Nov. 1890 and thought it pretty bright, as it was fairly conspicuous with a power of 60. Other discrepancies in the notes of observers might be alluded to. Dreyer says Hind's nebula (N.G.C. 6760), 4° W. of ν Aquilæ, "was suspected of variability, because it was refound by D'Arrest in May 1852 and called a first-class nebula, and subsequently 'pretty faint' and 'diluted,' while Auwers found it once 'pretty bright,' another time of the second class at most. There is, however, no reason for thinking it variable. It has little or no condensation, which probably makes its appearance more depending on the state of the atmosphere than would otherwise be the case." I swept up this nebula on Sept. 9, 1890, and called it "pretty bright, pretty large, brighter in the middle." In this and in many additional cases inconsistency in the records, instead of implying physical change in the objects, point rather to atmospheric variations, which are fully capable of introducing the discordances observed.

Those who have spent entire nights in celestial observation will often have noticed rapid changes in the condition of the atmosphere. It is seldom the air remains equable for a long period. Two nights are rarely alike, and though mere eye-observation detects little difference, telescopic power will often reveal great change. The opacity and pureness of the air vary from hour to hour. Some nights are unusually dark for several hours, the stars are very lucid and sparkling, and faint objects become intrusive. At other times the air is comparatively light, though there is no moon, twilight, or aurora of ordinary kind. The whole sky is occasionally suffused with a gauze of whitish material, seriously interfering with observation of faint objects. This is sometimes remarked as a striking feature. When watching the Perseids on several following nights in August 1880, the firmament glowed with a light that seemed stronger than the twilight of midsummer, but with this distinction, that in the former case the whole sky appeared equally affected. The transition from a dark to a light sky is sometimes quickly produced, and it is rarely that the air maintains a similar degree of transparency during an entire night. These changes are not caused by fog or mere surface phenomena, but are rather due to conditions affecting the upper limits of the atmosphere. I mention the circumstance as one bearing on the visibility of faint objects, which depends in great measure on the darkness of the sky contiguous to them.

Bristol, 1891, April 13. W. F. Denning.

Selenographical Notes.

Barrow, W. C. Bond, &c.—The borders of both these walled-plains exhibit that curious tendency to parallelism of which northern lunar latitudes afford so many examples, though in the case of Barrow it is far less marked than in that of its much larger neighbour, W. C. Bond, which, being wholly bounded by nearly straight walls, has the appearance of a vast rhomboidal enclosure. It is especially noteworthy soon after sunrise, both for the beauty and variety of its shadow-phenomena and the peculiar character and tone of the floor, then displayed to the best advantage. The most suitable time for observing these features is when the western side of the large bright ring-plain Timæus is on the morning terminator. This is also the best phase for examining the wall and other characteristic details of Barrow which render it a very interesting object under a low morning Sun.

Neison describes Barrow as a "nearly square-shaped walled-plain with moderately high walls," which, according to him, rise to 9419 feet on the east, and at the peak β on the west to a height of 7910 feet; but it is really a five-sided formation with long and nearly parallel borders on the north and south (the latter terminating on the east at the western side of the deep crater Barrow a); two equally straight but much shorter borders on the west, which, failing to meet, leave a notable gap in the wall; and a fifth section, also short but very broad, running from the eastern end of the northern straight border up to the northern side of Barrow a, thus completing the circumvallation. The continuity of the southern border is broken by one large depression B (Neison, Map VI.) and by other irregularities. On January 17, 6^h to 7^h, I noted a very bright little crater on the summit of this wall some distance east of B, which, strange to say (as it is a very easy object), is not shown by Schmidt or others. Between this and Barrow a the wall is perfectly straight and exhibits no detail whatever. The existence of the wide gap in the western border is beautifully manifested at sunrise, when the wedge-shaped strip of illuminated floor due to the opening is interposed between the black shadows of the wall on either side of it, which cover the remainder of the interior. As sunrise advances, a fine view is obtained of the shadow of the lofty peak β on the southern side of the gap, which at 6^h 30^m on the above date extended to the foot of the north-western wall of Barrow a. The mountains on the northern side of the opening must, however, be fully as high, as the mass of shadow due to them equalled that of β in length.

On the north-east side of Barrow, abutting on the wall but not penetrating it, is a very interesting example of a deep narrow rill-valley. It runs for some distance in a north-easterly direction, and ultimately curves concentrically round the foot of the slope of the north wall of Goldschmidt. Schmidt alone shows it. Neither Mädler nor Neison refer to it. It is a beautiful object under a high power, and seems to be due to the presence of a row of inosculating craters gradually diminishing in size from the foot of the wall of Barrow to the termination of the valley north of Goldschmidt. Another noteworthy circumstance in connection with it is that it is bordered on either side by a bank of some altitude, which was a very obvious feature on January 17. There is a prominent crater situated at the foot of the western bank and close to the bottom of the northern slope of Barrow, which does not appear in the maps.

The western side of W. C. Bond consists of an irregular double range of mountains of considerable altitude, extending from the neighbourhood of the deep crater B in the interior to the north side of Archytas. It follows a slightly serpentine course without, however, deviating to any great extent from a direct line. Two peaks on the more easterly range throw magnificent spires of shadow across the floor at sunrise, which, with the almost equally imposing shadow of W. C. Bond B, make up a very striking telescopic picture. Schroeter (Tab. lxi. fig. 1) shows a cleft running from the north wall of Archytas along the west side of the mountains just referred to, and Schmidt draws a coarse crater-row in nearly the same situation, both probably representing a valley between parallel heights. Beyond the western border is a large enclosure, Archytas C, bounded by hills of no great altitude, except on the extreme west, where they culminate in a very curious triple-peaked mountain. Neither Mädler, Neison, nor Schmidt show any detail in connection with W. C. Bond B, though a $3\frac{1}{2}$-in. achromatic reveals a good-sized crater on the floor close to it on the north. There is another just under the west border, also an easy object, shown only by Schmidt, and he represents a third on the south-west wall of β, of which I have no record.

Beaumont House, Shakespeare Road, THOS. GWYN ELGER.
Bedford, 1891, April 18.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

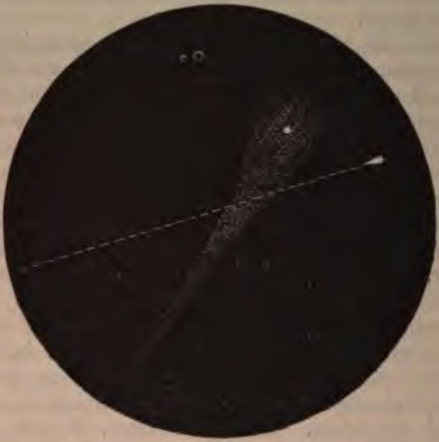
Comet a, 1891.

GENTLEMEN,—

While comet-seeking on the evening of March 30 at 9^h, with a 10-inch reflector, power 40, I picked up a bright nebulous object in Andromeda. As I failed to identify it with any of the conspicuous nebulæ which had become familiar to me during previous sweeps in this region, I concluded it to be a comet. A few minutes sufficed to reveal motion. The new comet appeared round, with a bright central condensation, but it was unfavourably placed in the mist over the N.N.W. horizon, and details were uncertain. At $16^h\ 30^m$ I reobserved the object, and found it very obvious, though the gibbous Moon was up. The daily motion I judged to be 70′ to the S., or more exactly S.S.E.

Another observation was obtained on March 31, 8^h 30^m, but the comet was indistinct in a hazy sky.

On April 4, 8^h 30^m it was again seen, and the atmosphere being unusually clear, the comet appeared much brighter than on previous occasions. It had a delicate tapering tail, so exceedingly diaphanous that it could only be caught by glimpses. It stretched over certainly more than half the field of 50′ (power 60) which I was using, though its precise limits were indeterminate, perhaps owing to the faint twilight lingering in that quarter. At 8^h 35^m a telescopic meteor, about 8th mag., crossed the comet just under the head, and the effect produced for a moment was very interesting.



Comet and Meteor, 1891, April 4.

Cloudy weather ensued, and the comet was not seen again. It is now lost in the Sun's rays. Prof. Barnard, of the Lick Observatory, appears to have discovered the same body on the evening of March 29, though my telegram conveyed the first intimation of the new object to the chief observatories in Europe. The comet is passing rapidly southwards, and it is to be hoped that its course will be watched from the southern hemisphere when it emerges from the Sun's rays at the latter part of May.

It is singular that it eluded detection until it had drawn so near its disappearance to northern observers. The comet would probably have been found here early in March but for extremely unfavourable weather. As an example of how unsuitable our climate is liable to be for this class of observation, I may mention that between March 3 and April 11 comet-seeking was only practicable on two nights.

Yours faithfully,

Bishopston, Bristol, 1891, April 13. W. F. Denning.

Is ι Cancri a Variable Star?

Gentlemen,—

This star is included by Mr. Gore in his useful list of Suspected Variables, where, however, he does not mention all the grounds on which its variability would seem to be really probable, though if so, the change must be of long period. Ptolemy and Al-Sûfi give its magnitude as the fourth, and it is nearly of that magnitude still, Prof. Pritchard photometrically determining it in 1883 as 4·24, nearly the same as that of δ, which agrees with Mr. Gore's estimate in 1876. But Flamsteed registers the magnitude as 5, Lalande made it the same, Bradley even fainter, as 5·6. Piazzi gives it also as 5·6; by a misprint the Bayer letter appears in his catalogue (where its number is viii. 158) as υ instead of ι. Argelander estimated the magnitude as the fourth—the same as it is at the present time; but the evidence appears pretty strong that it was smaller in the last century and at the commencement of this.

Yours faithfully,

Blackheath, 1891, April 8. W. T. Lynn.

Observing Occultations.

Gentlemen,—

In reading Mr. Innes's description of his visit to Mr. Tebbutt's observatory at Windsor, N. S. W., I was, I confess, reminded of the dictum of the American philosopher, that "There's nothing new, and there's nothing true—and it don't much signify;" for I orally described the method adopted by Mr. T. of observing occultations, before the Royal Astronomical Society more years ago than I at present care to count. Even after this lapse of time, I think that I can reproduce my remarks verbatim. I said then: "It may not be a very scientific method of observation, but it is a very effective one that I adopt. Adjusting the micrometer wire so that an equatorial star runs along it, I thread the star on that wire a short time before it is occulted, and leave the clock driving. Then when the star is about to reappear, of course I know exactly at what part of the Moon's opposite limb to look for it." I can only wonder that so very simple an expedient has not occurred to scores of people.

Faithfully yours,

Forest Lodge, Maresfield, Uckfield, William Noble.
1891, April 9.

Saturn.

Gentlemen,—

My attention has been called to a criticism in the 'English Mechanic' of my pictures of Saturn, and as this criticism is correct, I wish to acknowledge it. The inner edge of the gauzy ring is too bright. An attempt was made to stipple the stone, but in printing the colours ran together and gave a wrong appearance to the inner edge.

Anyone who makes a picture of a planet should bear in mind

Ben. Franklin's story of John Thompson the hatter, and take criticism kindly. The general tendency is to exaggeration. I may refer to an incident of my own experience. About ten years ago a book-agent visited this Observatory with some drawings of the planets on which very startling features were shown. On my referring to this fact, and saying that I had never seen anything so bold through the telescope, the agent, a long gaunt Yankee, drew himself up, and shaking his finger at me, said with a solemn voice, "*Professor, the people demand it.*"

Verbum sap.

Faithfully yours,
ASAPH HALL.

Naval Observatory,
Washington, D.C.,
1891, April 13.

OBSERVATORIES.

PARIS.—We gather from one of the Paris daily papers that Admiral Mouchez's request for an observatory outside Paris, either complete or at least sufficient for such work as is now seriously hampered by the situation, has been seized upon as an opportunity for proposing the demolition of the present observatory. It need scarcely be said that no such work of destruction is contemplated by Admiral Mouchez. He proposes converting the historic building into a museum if it is no longer to be used at all. But some energetic people point out that the building stands in the way of a very nice project for a sewer; and further that it never was a good observatory, and so had better come down. The story of the building is certainly rather curious, though we are sorry it has been remembered for such a purpose as that of urging its destruction. Louis XIV., being desirous of encouraging Astronomy, appointed Cassini his astronomer, and directed the architect, Perrault, to have an observatory ready for him on arrival. The architect made a fine building, but when Cassini arrived he found it would not do for astronomical purposes, and he accordingly built himself a small hut to observe in on the terrace; and Perrault's building has practically never been used as an observatory. The instruments and the Director have been established in pavilions and buildings successively added as occasion required.

PUBLICATIONS.

ASTRONOMY IN 1887 AND 1888 *.—There is so much life and activity in astronomical matters that it is a real pleasure to take stock once in a way; and such reviews as Prof. Young's 'Ten Years' Progress in Astronomy' are both instructive and entertaining.

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* An account of the Progress in Astronomy for the Years 1887, 1888, by W. C. Winlock. Washington, 1890.

We can even afford to look back a little oftener than once in ten years, as do the Royal Astronomical Society in their annual notes on progress; and Mr. Winlock has for some time past edited an annual review on the other side of the Atlantic. His work has, however, recently failed in one important feature of a good annual review: that of appearing to date, or as soon as possible after the termination of the period considered. Thus the report for 1886 appeared in 1889; and we have just received that for the years 1887–8 together. This may be due to the accidents of publication, as the report is included in the Smithsonian reports, but it is a pity some remedy cannot be found. The report itself is more or less a compilation, and there should be no difficulty in having it ready at the end of the year, if clippings are made at once. In the present case the treatment of two years together has led to the omission of the events of one (and it is not always clear which) in cases such as the reports of Observatories, award of medals, &c., &c. With this exception the work is carefully done, having probably gained in quality by the delay. There are many particulars of American Observatories which we have not had access to elsewhere, and the delay in publication does not of course affect the value of the review as a work of reference, as which it will be very valuable.

BIBLIOGRAPHY OF METEOROLOGY.—Referring to the Bibliography of Meteorology, a classified catalogue of the printed literature of meteorology, prepared under the direction of General Greely, of the Washington Signal Office, a notice concerning which appeared in the 'Observatory' for March, 1890, and of which parts 1 and 2 of the Section "Physics of the Atmosphere," dealing respectively with Temperature and Moisture, were then published, we have now to acknowledge the receipt of part 1 of the section "Mechanics of the Atmosphere," which deals with the subject of Winds. This is reproduced by lithography, for limited circulation, as before. General Greely regrets that he is compelled to produce this valuable bibliography in such a fragmentary and unsatisfactory form, but no funds are yet available for printing the Catalogue, and parts are issued in this form because no better method has been possible for making the work accessible to those who have aided in its preparation. W. E.

HANDBUCH DER GEOGRAPHISCHEN ORTS-BESTIMMUNGEN AUF REISEN*.—Dr. Wislicenus has noticed that travellers pay only scant attention to the determination of good geographical positions; and among other reasons for this omission he is convinced that one is the want of a text-book on the subject—a book, that is to say, which shall contain just all they ought to know and no more. He has done his best to supply this text-book in the work before us, which contains 270 pages octavo; which number of pages is hardly reassuring to the traveller who is anxious not to spend unnecessary time on this one of the many things that claim his attention. On glancing through the book, however, we cannot see that much could well have been omitted if the matter is to be dealt with fully. Of course a much smaller work might be produced in perhaps a few pages only; and we should think the author would have done well to add one or two chapters containing simple but definite programmes for a traveller equipped in definite ways—say, with a sextant and a good watch, or a theodolite and chronometer, or a theodolite alone. The chief danger in making a text-book so complete is that the choice of methods offered is merely bewildering; take for instance the dozen or so methods of finding the latitude here given; each is most clearly and concisely explained, and a good example fully worked ont. But there is very little to indicate which method should be chosen in particular cases.

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* Handb. u. s. w. zum Gebrauch für Geographen und Forschungreisende; von Dr. W. F. Wislicenus. Leipzig: W. Engelmann.

There is another point in which the examples might have been made more useful to those unfamiliar with the methods. One of the things about which a beginner is most in doubt is whether his observations are sufficiently accordant; and if Dr. Wislicenus had given in each case a series of four or five results obtained by a practised observer, like himself, instead of only a single one, a very useful standard of accordance would have been established.

It is, however, somewhat ungracious to complain of what the author has not given us when he has already been so generous. The great merit of the work is its completeness and clear style of writing; and we must not forget to mention the carefully minute directions to intending observers about such things as lamps and notebooks &c. Attention to these words of wisdom, from even some experienced astronomers, would save many hours of subsequent labour. The work is in three parts: the first forty pages deal with spherical astronomy; the next hundred with instruments; and the last part with the actual methods for determining latitude and longitude. A very slight knowledge of German would be sufficient for understanding this last and most important part of the work.

CELESTIAL MOTIONS *.—We are glad to see another edition (the seventh) of this admirable little work, which has already been translated into French.

THE SOLAR PARALLAX AND RELATED CONSTANTS.—The great work which Prof. Harkness announced only recently (see 'Observatory,' No. 173, p. 133) is already completed and published. The whole occupies 160 pages of the Washington Observations, and forms Appendix III. to the Volume for 1885. We must defer any extended or critical notice of the work; but may here remark that

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* 'Celestial Motions, a handy book of Astronomy,' by W. T. Lynn, B.A. London: Stanford. Price 2*s.* 6*d.*

the value obtained for the solar parallax by this exhaustive discussion of all existing observations is 8″·81 ± 0″·006, and of the constant of aberration 20″·455; and for the semidiameters of the Earth 20925293 feet and 20855590 feet respectively, giving a flattening of 1/300·2 ± 2·96.

THE ASTROPHOTOGRAPHIC CHART: 6TH NO. OF THE BULLETIN.—This number, which appeared just before the meeting of the Committee, opens with a paper by M. Kapteyn on the parallactic micrometer, whose construction has been rendered possible by the private munificence of M. Bischoffsheim; and a more technical paper on the same subject by M. P. Gautier. The theory of reduction of the measures is expounded by M. Kapteyn in another paper occupying half the number (50 pages). The merits of the apparatus can in many ways be more profitably discussed when it has been made and used; and we accordingly shall not consider them at present.

A paper in which M. Prosper Henry points out the change of the constant of refraction in photographic work has been already published and referred to: and there is little else in the number calling for particular comment.

NOTES.

A FIFTH TYPE OF STELLAR SPECTRA.—Prof. E. C. Pickering, writing in the 'Astronomische Nachrichten,' No. 3025, proposes this title for the stars whose spectra resemble those of the stars discovered by MM. Wolf and Rayet. In general Prof. Pickering's photographic survey has confirmed Seechi's fourfold division of stellar spectra; but many stars in Orion and the neighbourhood differ considerably from the ordinary first type stars, the additional lines instead of being faint as in Vega, being nearly as intense as the hydrogen lines, whilst two classes of objects, the planetary nebulæ and the stars the spectra of which consist chiefly of bright lines, are left unprovided for. Prof. Pickering points out the close similarity of the grouping of the lines in these three classes, and also the striking character of their distribution. Whilst stars of the second and third types are about equally divided between the Galactic and non-Galactic regions, two-thirds of the first-type stars lie in the former area, and of the Orion stars four-fifths are found in the Milky Way. A similar distribution of the planetary nebulæ has long been recognized, and Prof. Pickering here shows that of 33 stars known as of the "Wolf-Rayet," or, as Prof. Pickering suggests they should be called, the "fifth" type, every one lies within 10° of the Galactic equator, two-thirds within 2° of it.

The following table shows the correspondence between the lines of the Orion stars and those of the fifth type, in which Prof.

Pickering proposes to include the planetary nebulæ. The Wolf-Rayet stars he divides into three classes, taking Cord. G. C. 8631, Cord. G. C. 22763, and S.D. 21° No. 4864 as typical examples. In the first two classes λ 469 is the most conspicuous line; in the third, λ 464.

Orion Stars. (Dark lines.)	Planetary Nebulæ.	Bright-line Stars.		
		Class I.	Class II.	Class III.
—	501	—	—	—
486	486	486	486	—
470	470	469	469	—
463	–	462	464	464
454	—	454	455	455
451	—	—	451	451
447	447	—	447	—
442	—	—	—	443
434	434	434	434	434
420	—	420	420	421
410	410	410	410	412
407	—	406	406	407
403	—	402	402	—
397	397	398	397	—
394	—	395	—	395
389	388	389	389	—
387	—	388	—	—

ROTATION OF JUPITER.—In a paper read to the St. Petersburg Academy in November last, Dr. Belopolsky has collected and discussed all available observations for the rotation of Jupiter. The result is to confirm the existence of two distinct periods of rotation for the visible Jovian surface, one of about $9^h\ 51^m$ attaching to an equatorial belt; and the other of $9^h\ 55^m$ for the rest of the surface, at least as far as latitude 45°. The equatorial belt is well defined as far as 5° on either side of the equator; but from this point to 10° or 15° visible markings may belong apparently either to the belt or to the rest of the surface, and give results for rotation period accordingly. The belt does not appear to be quite symmetrical with respect to the equator, and if it corresponds to the belt of trade-winds on the Earth must have some additional peculiarity not represented on the Earth; unless, indeed, the unequal distribution of our land and water in our two hemispheres is the cause of some asymmetrical optical phenomenon to observers in other planets.

THE AURORA.—Prof. F. H. Bigelow, who has recently paid some attention to the mathematical theory of the solar corona considered as an electrical phenomenon, is anxious to make a similar analysis of the problem of the aurora; but the proper observations are lacking. He begs those who have the opportunity to make the simple observations required, which consist in measuring the angle of inclination of a streamer to the vertical plane passing through the station, together with the azimuth of the ray, prolonged, if necessary, at the point of its springing from the horizon. His paper, in which he describes a simple instrument for the purpose, will be found in Amer. Journ. of Sci. vol. xli. Feb. 1891; and in case any of our readers wish to make such observations, we shall be glad to forward them a copy.

THE ALLEGED VARIATION OF LATITUDE.—In Astr. Nach. 3021, Dr. N. Herz suggests a new origin for the variation of latitude in the electrified state of the Sun. If the Earth be not an absolute conductor nor the air an absolute non-conductor, it is shown that the direction of gravity at any point will thus apparently change, not only with the season of the year, but during the day; while its intensity need not be sensibly affected. It is pointed out that if this be the true cause, the observed phenomena should vary with the latitude, and not with the longitude, as would be the case for a real shifting of the Earth's pole; and we thus have a means of putting Dr. Herz's theory to a thoroughly practical test.

A VARIABLE NEBULA.—The nebula N.G.C. 1186, which was discovered in 1785 by W. Herschel and observed in 1831 by his son, could not be found in 1854 and 1864 by Lord Rosse, nor in 1863 by d'Arrest; but has been lately observed by M. Bigourdan at Paris (C. R. cxii. no. 9, p. 471). There is thus a strong probability that it is variable, and it certainly deserves attention.

MINOR PLANETS.—It appears that the Minor Planet announced on p. 136 of No. 173 as discovered on Feb. 11 is Lacrimosa No. 208; the Nos. on pp. 136 and 166 should therefore be decreased by 1. No. 308 was discovered by M. Borrelly on March 31 and No. 309 by Dr. Palisa on April 6. No. 303 has been named Josephina.

THE NEW FRENCH BASE-LINE.—The officers of the French "Service Géographique" measured a new base-line in the summer of 1890, and the Director, Gen. Derrécagaix, has just communicated the result to the Academy. The work was obviously done with great precision and also great rapidity, owing to efficient organization. The base selected was nearly that of Picard, between Villejuif and Juvisy, though it was not possible to use Picard's terminals, one of which is now surrounded by buildings. It was measured twice (the operations extending over 25 days the first time and 18 the second), and the greatest difference between the two measures of a section of 200 or 300 metres was $2^{\text{mm}}\cdot6$, the final result for the whole base being $7226^{\text{m}}\cdot792$. On comparing this result with Delambre's base, Melun—Lieu Saint, through the triangulation, the difference is found to be only $0^{\text{m}}\cdot01$; but on comparing by triangulation with the fundamental linear measures of other countries, a curious systematic discrepancy appears as follows (the first number being the excess of the foreign measure over the French, and the second the whole base-length):—

	m	m
Spain (Forceral—Canigou)....	—0·44	30141
Belgium (Kemmel—Cassel) ..	—0·31	22982
England (Harlettes—Cassel) ..	—0·68	37460
Italy (Mounier—Tournairet) ..	—0·46	26010

The reason for this discordance is yet to be determined. We congratulate the Service on the promptitude with which such an excellent piece of work has been accomplished.

PHOTOGRAPHIC STELLAR MAGNITUDES.—The method proposed by Capt. Abney for determining stellar magnitudes photographically is worthy of careful attention. Hitherto various empirical formulæ connecting the brightness with the diameter of the disc have been suggested with more or less success. Capt. Abney deals with the total photographic action, including both the density and the size of the image. He measures, in fact, the total obstruction to light offered by the image when it is placed as a screen in front of an aperture. It can in this way be compared with a scale of screens or a graduated screen and the total action measured; and the relation between the total action and the brightness of the source has been specially and successfully investigated by Capt. Abney. We hope his beautifully simple and complete proposal will meet with approval.

MESSRS. TAYLOR AND FRANCIS will shortly publish a new illustrated book, intended for astronomical amateurs, and written by Mr. Denning, of Bristol. The title is 'Telescopic Work for Starlight Evenings,' and the volume will include chapters on telescopes and observational matters, in addition to descriptions of the principal celestial objects.

AN appeal is being made in California for funds to enable Messrs. Burnham and Keeler to take the 12-inch Equatorial to Chile, on the occasion of the favourable opposition of Mars in 1892, when the planet will be in Declination 23° South, and thus badly situated for observation in northern latitudes.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 176. JUNE. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, May 8, 1891.

Lieut.-Gen. J. F. Tennant, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. Knobel and A. M. W. Downing, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Downing. Seventy-seven presents have been received since the last Meeting; amongst those calling for special mention are:—E. Dunkin, 'The Midnight Sky,' new edition, presented by the author; C. A. Young, 'Lessons in Astronomy,' presented by Mr. E. Arnold; Pulkowa Observatory, Catalog von 5634 Sternen, presented by the Observatory; Munich Observatory, Annalen, Band I. Erstes Münchener Sternverzeichniss von 33,082 Sternen, presented by the Observatory; Photograph of the Cluster in Perseus made at the Lick Observatory, presented by Prof. E. S. Holden.

A vote of thanks was accorded to the donors of the presents.

Mr. Knobel read a paper by *Mr. K. D. Naegamvala* "On the character of the chief line in the Nebula in Orion." Mr. Naegamvala has been observing the spectrum of the Great Nebula in Orion on every available occasion for two months at Poonah in India. The telescope used was a 16½-inch reflector by Calver, and spectroscopes having one, two, and three prisms have been used. The results of the observations are that the chief nebula line is seen sharp under all circumstances, and no trace of any fluting structure has ever been seen. It is therefore concluded that the line in question is not the remnant of a fluting of magnesium, as has been suggested by Prof. Lockyer in his 'Meteoritic Hypothesis.'

Captain Noble. I think it cannot fail to be gratifying to our eminent Fellow, Dr. Huggins and his gifted wife, to find that his conclusions have been corroborated on the American Continent by Mr. Keeler, in Germany, and now in India. The character of this line has been under discussion for two or three years, and, despite evidence to the contrary, it has been insisted upon that it has its origin in magnesium. I think Mr. Naegamvala has—I will not say killed—but buried the idea.

Prof. Alexander Herschel. There has been a great deal of research expended upon this line since it was first discovered, when it was said to be due to nitrogen. Even now we cannot say definitely what its chemical origin is, and we must wait and hope for a solution of the question. But this does not touch the main point of Mr. Lockyer's hypothesis, which is that the luminosity of nebulæ is due to low temperature. There is a background of uncertainty as to the chemical origin of the line, and contradictory accounts of its appearance, and the question of the true source of nebulous light is one that remains for the future to decide.

Mr. A. Taylor. In 1888 I examined the spectrum of the Great Nebula in Orion, and the conclusion that I came to then, and which I communicated to the Society in a paper in January 1889, was that this line was sometimes seen fluffy on the edge towards the blue. A similar observation has been made in nebular spectra by Dr. Huggins on several occasions, by Father Secchi, Dr. Vogel, Dr. Bredichin, and Mr. Maunder, and the appearance of this fluffy edge is not by any means confined to one nebula. It has been seen so repeatedly that a single observation now cannot dispose of it, and we must seek for a full explanation. The earlier observations were allowed to stand unexplained until Mr. Lockyer brought forward his meteoritic hypothesis, and suggested that the line was a remnant of the magnesium fluting. Since that suggestion was made Dr. Huggins, Dr. Vogel, and Mr. Maunder have said that they see the line perfectly sharp; but this does not invalidate their previous independent and unbiassed observations. Their first records were made to publish facts of observation the importance of which they did not recognize, and no observations made by them now will carry so much weight as their previous unprejudiced work. Dr. Huggins and others say they now see this line perfectly sharp, and in my own observations I find that I can sometimes see it practically sharp, and at other times fluffy. So far as I can make out, everything depends upon which portion of the nebula is falling on the slit of the spectroscope. It is a great disadvantage that observers, when they are examining nebular spectra, will not record which portions of the image of the nebula are falling on their slits, then we should be able to get definite evidence as to differences in the spectrum, and perhaps to understand how it is that this line is sometimes seen sharp and sometimes fluffy.

There is another point, and that is with regard to the observations of Mr. Keeler at the Lick Observatory. Much importance is attached to the fact that he, with large aperture and great dispersion, does not see any trace of a fluting. As a matter of fact, his conditions are nearly the worst possible for making this observation. For stellar work his large aperture would be an advantage, but for the examination of nebulæ and other objects of sensible magnitude the large aperture is a positive disadvantage if there is a corresponding increase of focal length; and the Lick telescope of 36 inches aperture and 672 inches focus is quite unfitted for this work. A 12-inch telescope of 18 feet 8 inches focus would be just as effective, and if of shorter focal length, would be better than the Lick telescope. For observing and measuring a sharp line, or *the sharp edge* of a fluting, great dispersion is an advantage, but a small dispersion is more fitted to detect the faint light that fringes the edge of a fluting. Great dispersion spreads the faint light out until it becomes invisible. Besides this, with a grating spectroscope the loss of light is very great. When we add to these disadvantageous conditions the fact that Mr. Keeler's eyes are not acute, we can easily understand how it is that he cannot see any resemblance to a fluting in the nebula line. I should have been very surprised indeed if he had, for his telescope, his spectroscope, and his eyes are all against him. His negative evidence has had too high a value placed upon it, for negative evidence from him is of very little value, although positive evidence from him would be of greater value than from anyone else. In the paper read to us to-night, although we are told the aperture of the telescope, we are not told its focal length, and no mention is made of the portion of the nebula examined, so that we cannot possibly determine the value of these observations.

Captain Noble. I should like to ask one question, and that is, does Mr. Taylor ask this meeting to believe that he contends there is still a chance of this chief nebula line having its origin in magnesium?

Mr. Taylor. I do not think that I ought to be cross-examined on this question, or that we are in any way discussing my opinion. We are discussing facts of observation, and they are that Dr. Huggins, Father Secchi, Dr. Vogel, Dr. Bredichin, Mr. Maunder, Prof. Lockyer, Mr. Fowler, and myself have on separate occasions seen this line fluffy on the blue edge, whereas the writer of the paper sees it sharp. My opinion of the chemical origin of the line has nothing to do with the matter at all. This much I will say, however, that the observations of Mr. Keeler have certainly made the suggested magnesium origin more probable than the nitrogen origin that was accepted for so long, or the lead origin, if Dr. Huggins ever believed in that. I see no reason for doubting Mr. Keeler's measurements, and on some occasions he has measured the magnesium line to the red side of the chief nebula line, although his mean results show that it is on the other side. Motion in the line of sight, a swirl in the nebula, or instrumental errors may account for the differences, but in any case his results strengthen the suggested magnesium origin, and completely demolish all other suggestions.

Mr. Ranyard. I should like to point out that in the whole course of observations of motion in the line of sight there is a shifting of the line as a whole, and no tendency to produce a nebulosity on one side only. I do not see how any such effect could be brought about. We might get nebulosity on both sides, but not on one. Some solar lines were believed to be unequally winged, but with more powerful instruments and better definition this is found not to be the case. One thing we can rely upon is the position of the line. The nebulosity of the edges is much more difficult to observe.

Mr. Taylor. It is quite possible that the nebulosity on one side of the line may be due to motion in line of sight. If some of the luminous matter is at rest and another portion approaching us in the line of sight the result will be a sharp edge on the red side and a fluffy edge on the blue. This widening on one side can sometimes be seen with the hydrogen lines of the Sun, and it may be that this fluffy edge to the line in the nebula in Orion is an effect of a similar cause. It may be due to this or it may be the remnant of a magnesium fluting, but there can be no doubt of the real existence of this fluffy edge to the line.

Dr. Gill. There is one point with regard to the observations of Mr. Keeler that I should like to mention. It is perfectly true all that Mr. Taylor has said on the subject of the proportion of focal length to aperture of the telescope, but in this case I was not aware that there is any difficulty in seeing the line. It is easily visible. I am not a specialist, but there can be no doubt that if you are observing a continuous spectrum, as from a star, a large dispersion would dilute the light, but in the case of monochromatic light the large dispersion used by Mr. Keeler would be an advantage for accurate measurement of the wave-length of the line. The point on which the identity of the chief nebula line with magnesium turns, *i. e.* the question of wave-length, is whether the nebula has a motion in the line of sight, of about 36 miles a second. If there is one question more certain than another in celestial spectrum analysis, it is that hydrogen is a constituent of this nebula. Now the hydrogen lines do not show any such motion, so that the evidence is against the magnesium origin. I speak from general principles and not as a specialist.

Mr. Gregory. I think that the suggested magnesium origin hangs as much upon identity in appearance as coincidence in position. The nebula line was thought, until a few years ago, to be due to nitrogen (although nitrogen gives a double line here), and it was only after Mr. Lockyer suggested that it might be magnesium that Mr. Keeler's observations were made, and they were made with the idea of disproving the suggestion. Mr. Lockyer's suggestion is at any rate nearer the truth, and entitled to more respect than Dr. Huggins's suggestion of nitrogen, and Mr. Keeler's observations have strengthened the idea of the magnesium origin.

Mr. Maunder. I observed the principal line in the nebula in

Orion in March 1884, and that observation has been referred to several times to-night. On that occasion I used two half-prisms, equal to about 16 ordinary prisms, and the nebula line did not give me the idea of being the remnant of a fluting. It was a line, but was not sharp on either edge, and the edge towards the blue was a little more diffuse or a little more fluffy than the other one. I do not follow Mr. Lockyer's idea that a large dispersion will be less likely to show this than a small one. I have observed the line with smaller dispersion and have never been able to detect any lack of sharpness or want of symmetry except on the one occasion.

The President. Mr. Taylor says that different parts of the nebula give different characters to the line. I should like to know if he considers it probable that the magnesium is present in some parts and absent in others. If the fluffy edge is due to motion in line of sight, then some parts are in motion and others at rest. That is what the suggestion amounts to. Does he attribute part of the line to magnesium and part to some other cause?

Mr. Taylor. I can only give the facts of my observations, and they are that the character of the line varies when different parts of the nebula fall on the slit. I find that when the trapezium and the condensed portions of the nebula near it fall on the slit the extension towards the blue is greater than is seen in any other part of the nebula. Whether this is due to the stars in the trapezium being condensations in the nebula, whether there is more magnesium there, or whatever the cause is I cannot say.

Capt. Noble. Dr. Huggins photographed the spectrum one night with his slit across the trapezium, and his photograph ought to show this.

Mr. Taylor. Dr. Huggins has not photographed the 5005 line. His plates will not reach it. I may say that there are photographs in existence, but not yet published, that seem to indicate a swirl in the nebula such as I have indicated. Drawings from and descriptions of these plates will be published, and copies of the actual photographs will, I hope, be available for examination.

The President called upon *Dr. Gill* to give an account of his visit to the Continent.

Dr. Gill. My visit to the Continent was chiefly connected with the Astrophotographic Congress. I had the pleasure of reading in the 'Observatory' the exceedingly accurate account of the Congress given by the Astronomer Royal, therefore I need not refer to what has been told you already. After the Congress we had to remain behind to complete the revision of the *procès-verbaux*, and after a pleasant stay in Paris I started for Berlin. There I was principally occupied in discussions with Dr. Auwers with regard to the possibilities and probabilities of getting certain meridian observations of the comparison stars which will be required as standard stars upon the photographic plates for the formation of the catalogue. I forget whether the Astronomer Royal told you of the decision of the Committee that there were to be six stars, chosen as symmetrically as possible, on each of the plates for the catalogue. The positions of these stars are to be determined by meridian observations at two observatories, and by two observations at each observatory. That involves the preparation of a catalogue of between 60,000 and 70,000 stars over the whole sky, and that catalogue would have the advantage that these stars would be distributed as evenly as possible over the whole celestial vault. I believe that it will be possible, with the sympathy and co-operation of astronomers, to prepare this catalogue and to obtain in the course of a reasonable period a complete re-observation of the selected stars. The first step will be to select the stars, and that selection will have to be made by laying down upon the chart of Argelander a square equal to each of the 10,000 plates, and to select the six stars most suitably placed in each plate. Argelander's 'Durchmusterung' and Schönfeld's extension of it will enable this to be done for the northern hemisphere and down to 23° South declination. We hope in the course of another year that the catalogue of the Cape photographic 'Durchmusterung' will be completed, when we shall be able to lay down the stars in charts; from these charts the stars for the rest of the southern hemisphere may be selected. From Berlin I went to visit Messrs. Repsold at Hamburg, and saw Dr. Vogel's apparatus for measuring the star-plates, which is extremely conveniently contrived and well made; and the same may be said of a new heliostat for Potsdam. From there I went to Groningen and discussed with Prof. Kapteyn details as to how the catalogue and charts of our 'Durchmusterung' should be published. I only hope I shall be able to get the necessary funds as quickly as possible. I should have said that during my visit to Berlin I spent two days at Potsdam with Prof. Vogel, and returned deeply impressed with the extreme accuracy and importance of the observations going on there for the measurement of rate of motion of stars in the line of sight.

Dr. Gill then described the spectroscope used by Prof. Vogel by the aid of drawings on the blackboard. A strong conical cage of steel T-rods carries the collimator and is bolted on to the tube of the telescope. The two compound prisms used are mounted in a steel case accurately adjusted for minimum deviation of "G," and then rigidly clamped. This is bolted on to the case carrying the collimator and is perfectly rigid. The photographic camera is mounted on the end of a steel conical tube, and this is in turn bolted to the prism case. A strong steel arm ties the end of the camera case to the tube of the telescope. When thus mounted the whole spectroscope is incapable of any accidental displacement, and is protected against alterations of the adjustments owing to motion of the telescope.

Resuming, Dr. Gill said: I was very much impressed with the apparatus, and having made a number of observations and measured a number of photographs of spectra which Dr. Vogel had obtained with it, came to the conclusion, which I believe is not an extravagant one, that the work done by Prof. Vogel with this apparatus is incomparably the most accurate yet done; so much so that with a 25-inch or a 30-inch telescope well mounted, and a spectroscope similar to this, employing the largest dispersion which can be used, I believe that by observation of the spectra of the brightest stars it will be possible to measure the velocity of the Earth in its orbit so accurately that the solar parallax will be obtained in this way more correctly than by any other method. That is an enormous advance in exact spectroscopic work, and I hope that Potsdam will not be long before it has a telescope capable of doing all I have suggested. I also hope that Potsdam will not be the only place where we shall see such results.

Mr. Taylor. Does Dr. Vogel use a cylindrical lens? and if so, where does he put it?

Dr. Gill. No, he does not use a cylindrical lens; but places his slit parallel to the motion in Right Ascension, and uses a slit of limited length. He then allows the star to wander backwards and forwards in the direction of the slit, and that gives the necessary width to the spectrum.

The vacuum tube for giving the Hγ line of hydrogen across the star spectrum is fixed across the tube of the telescope, some distance in front of the slit and at right angles to it.

The Astronomer Royal. I quite agree with Dr. Gill in his appreciation of the great accuracy of Prof. Vogel's results. I was surprised that such good results were obtained with the vacuum tube in that position, because it seems to me optically wrong. This method of placing the vacuum tube in the cone of rays and at right angles to the slit was adopted by Dr. Huggins, and was the method first used at Greenwich, but it was found necessary to give it up because it gave erroneous results. The rays from the vacuum tube and from the star were under different conditions in the collimator. Different parts of the object-glass of the collimator were illuminated by different parts of the vacuum tube, and in this way it became possible to get erroneous results. Our experience showed that the method could not be implicitly trusted, and we gave it up several years ago, though Dr. Huggins and Dr. Vogel seem to have obtained satisfactory results with it.

Dr. Gill. There may be other reasons for the failure than the position of the vacuum tube.

The Astronomer Royal gave an account of the new dome for the Royal Observatory, Greenwich, illustrating his description by a model of the dome and by lantern slides made from the drawings.

He said:—The new dome is intended to house the new 28-inch refractor, which is nearly finished, and as it presents some novel features I thought it might be of interest to the Fellows if I give a short account of it. A proposal was made to erect this dome in place of the wooden drum of 30 feet diameter which is now mounted on the south-east tower, and that proposal has been sanctioned by the Admiralty. The work of construction of the dome is being carried out by Messrs. Cooke and Sons of York, and I hope by the autumn that it will be erected and the 28-inch telescope housed in it. It was necessary to have the dome of greater diameter than the tower, and to avoid building out at the top of the tower it has been arranged to make the dome 31 feet diameter at the plane of the rail on the tower, and 36 feet in diameter higher up. The dome in section takes the form of two involutes of a circle joined together at the top by an arc of a circle. Another feature is the construction of the shutter, for I wished to have that made so that it shall be perfectly symmetrical. The shutter-opening extends right over the dome from the curb through the zenith to the opposite part of the curb, so that the dome virtually consists of two separate portions joined together by a flanged rib carrying a rail at the top. The opening between the two halves is 7 feet wide, and the shutter is divided down the middle and opens both ways. This double shutter equalizes the weight when the slit is open and prevents undue strain of the dome on one side. The top of the shutter runs on the rail at the top of the dome, the rollers being arranged both above and below the rail.

The telescope is 2 feet out of the centre of the dome, to the south of the axis of the dome, so that when pointing to the zenith it is clear of the rail. The mounting of the present instrument is to be retained, and the dome will be of sufficient size to allow the whole of the telescope tube to be in the dome in all positions except when the telescope is pointing very near the horizon on the meridian. The apparatus for rotating the dome will be similar to that used for the Lassell dome, the rollers having slightly curved faces running on the rail, this form of face being found very efficient and practically diminishing friction and risk of obstruction from grit to a minimum.

Mr. E. T. Carter. I know very little of the practical working of a dome, but I am familiar with the principles of rolling friction. The method of using rollers with curved faces is not the best for reducing friction when any lubricating material is used. The pressure is on a line, and not spread over a surface; the result is, that the lubricating material is pressed out, whereas if a surface-bearing were used, it would reduce friction.

The Astronomer Royal. We find the curved faces to the rollers act perfectly well with the Lassell dome. The difficulty in the case of a comparatively light dome is not so much to reduce rolling friction by lubrication as to prevent the grit which accumulates on the rail from interfering with the rotation of the dome. This is efficiently provided against by using the curved rollers.

Mr. Isaac Roberts then showed on the screen a photograph of the region of Hind's variable nebula in Taurus. Mr. Roberts said that Mr. Burnham, of the Lick Observatory, had examined this region with the 36-ft. refractor, and found a very small condensed nebula near where Mr. Hind's nebula was supposed to be. The photograph did not show any nebulosity, or nebula, or nebulous star anywhere about the region referred to, though the exposure extended during 3 hours, and therefore nebulosity of a very faint character should have appeared after such an exposure. He suggested that probably some rapid changes had taken place in the nebula seen by Mr. Burnham, otherwise it should have appeared on the photograph. The planet Neptune with its satellite were shown on the photograph, and though the satellite was so faint, its disc was a strong black spot on the film of the negative.

Photographs of Neptune and its satellite were shown. In all 16 photographs of the planet were taken between December 1890 and February 1891, with exposure of the plates varying between 15^{m} and 3 hours. The satellite was shown upon 8 of the plates, and a diagram was projected on the screen to show the positions of the satellite with reference to the planet on those 8 days. The star number 699 zone 19° of the Durchmusterung, magnitude 9·4, was partly covered by the planet on the 12th December, 1890, and the diagram showed the relative position of the planet, star, and satellite on that date. The photographs did not indicate the existence of any other satellite than the one discovered by the late Mr. Lassell.

Mr. Roberts also showed a photograph of the cluster 44 M Cancri (the Præsepe). The photograph was taken on February 13, 1891, and the enlargement presented was intended as a chart of the stars between R.A. $8^{h}\ 30^{m}$ and $8^{h}\ 38^{m}$, Declination north $19^{\circ}\ 19'$ to $21^{\circ}\ 19'$, covering 4 square degrees of the sky. The exposure was given during 90^{m} in the 20-in. reflector. No nebulæ were shown on the photograph.

Mr. Plummer gave a short account of *Prof. Pritchard's* paper on "Further Experiments regarding the Magnitude of Stars, as obtained by Photography at the Oxford University Observatory."

Notwithstanding all that had passed at the Paris Congress, and before and since, there still seemed to be great doubt as to the time necessary to produce a satisfactory impression of the fainter stars. For instance, the Astronomer Royal had, in a paper communicated to the Society in March, proposed an exposure of 90 seconds as sufficient to record an eleventh-magnitude star, while Prof. Pritchard had stated in a paper sent to the French Congress that 200 seconds were necessary. Still greater difficulty existed in determining the proper time for the impression of a fourteenth magnitude.

With a view to clearing up these discrepancies, a series of experiments had been made in the first place in the region occupied by Prof. Peters's charts, and again in the zone allotted to Oxford. In each case the stars impressed on the photographic plates were photometrically examined by the wedge, and the magnitudes accurately determined. The result was that the previous conclusions to which Prof. Pritchard had been led were completely confirmed; and therefore it must be accepted as a fact that a great difference in time did exist as necessary for the production of stars of a definite magnitude in various instruments. This difference, however, did not affect the arrangements for the Congress, which proposed that each Observatory should determine for itself the time required to produce a definite black image of stars of the eleventh magnitude. In connection with this subject Prof. Pritchard offered to determine the magnitudes of a number of eleventh-magnitude stars as a supplement to the method suggested by the French Committee.

With regard to the fourteenth magnitude, Prof. Pritchard described the means he had taken to ensure the production of such faint stars on his plates, and the difficulties encountered in accurately determining the magnitudes as photometrically conceived.

It had been stated very definitely that an increase in the intensity of light was exactly equivalent to a proportionate diminution in the time of exposure. This proposition it was impossible to controvert, because we could not estimate the whole of the molecular action. But if the diameter of the star-image were accepted as a criterion of the amount of actinic action, then the law did not hold. For since every magnitude differed from the next lower by two and a half times the light, it was evident that on a series of exposures being made for durations, each of which exceeded the one previous by $2\frac{1}{2}$ times the amount, the diameter of the consecutive magnitudes should be equal. For instance, if with an exposure of 20 seconds a star of the third magnitude had a diameter of $9''$, then on exposing for 50 seconds a star of the fourth magnitude should also have a diameter of $9''$, and on continuing the exposures $2\frac{1}{2}$ times this length each successive magnitude should have the same diameter. Prof. Pritchard showed that this law did not hold on his plates, and argued from this fact that it was unsafe to infer that a definite photometric magnitude must necessarily be impressed because the plate had been exposed over a certain time.

The following papers were announced:—

K. D. Naegamvala. "On the Character of the chief line in the Nebula in Orion."

The Astronomer Royal. "On a new Dome at the Royal Observatory, Greenwich."

Isaac Roberts. "Photographs of the Region of Hind's Variable Nebula in Taurus."

Isaac Roberts. "Photographs of Neptune and its Satellite."

Isaac Roberts. "Photograph of the Cluster 44 M Cancri (the Præsepe)."

Prof. C. Pritchard. "Further Experiments regarding the Magnitude of Stars as obtained by Photography at the Oxford University Observatory."

Alfred Henry Fison, D.Sc., University College, Gower Street, W.C.; *Thomas Gordon*, 9 Scotch Street, Whitehaven; *George E. Hale, B.Sc.*, 4545 Drexel Boulevard, Chicago, U.S.A., were duly elected Fellows of the Society.

The following Candidate was proposed for election to the Society:—

Oliver J. Lodge, D.Sc., LL.D., F.R.S., 21 Waverley Road, Liverpool (proposed by G. H. Darwin).

BRITISH ASTRONOMICAL ASSOCIATION.

THE seventh ordinary Meeting of this Association was held in Barnard's Inn Hall, Holborn, on May 27, 1891, Dr. W. Huggins, Vice-President, in the Chair.

The names of ten Candidates for admission were read and ordered for suspension, and twenty-four Members were duly elected.

Dr. Gill then gave an account to the Meeting of the principal decision arrived at by the International Astrophotographic Committee which met recently at Paris. One important result was, he said, the adoption of the réseau. Another was the decision as to the plates for the Catalogue. These plates were to each have two exposures, one long enough to give measurable images for stars of the 11th magnitude, the other just half as long, yet sufficient to show stars of that magnitude, though faintly. This decision placed the Catalogue beyond dispute. Then came the plates for the Chart. These were to extend to the 14th mag. But instead of defining the 14th mag. an arrangement was adopted which would secure a uniform system for all the observatories. Then further the Catalogue plates were to be pushed on with, but the Chart plates were by no means to be neglected. The Conference had laid the bases of a system of sidereal astronomy, for which our descendants 200 or 300 years to come would give us the heartiest thanks.

Dr. Gill then described the new equatorial coudé of the Paris observatory, and the most ingenious extension by M. Cornu of the "Cavendish experiment" for determining the density of the earth.

He next referred to the recent work of the Potsdam Observatory, and declared it as his opinion that it was not only possible but probable that with more powerful instruments the solar parallax might be independently determined from observations of the motions of stars in the line of sight. He had also visited Dr. Kapteyn at Groningen, who was measuring the plates Dr. Gill had taken at the Cape. Seventy per cent. of the work was already done, and probably by the end of the year the results would be ready in catalogue-form.

The Rev. A. Freeman read a paper on an eclipse of Titan by Saturn.

Mr. Cottam read two papers by Mr. Keeler and Mr. Stanley Williams on the Red Spot of Jupiter, with reference to M. Niesten's observations published in the February number of the 'Journal.'

Mr. Green spoke on the work proposed to be undertaken by the Saturn Section, of which he had been appointed Director; and Mr. Maunder read a paper on Stars of the First and Second Types of Spectra, in which he argued that difference of type rather meant difference of constitution than difference of age, and that on the average the Solar stars appeared to be larger than the Sirian.

MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

Wednesday, May 20, 1891.

Mr. Baldwin Latham, M.Inst.C.E., F.G.S., *President*, in the Chair.

The following papers were read:—

1. "On the Vertical Circulation of the Atmosphere in relation to the Formation of Storms," by Mr. W. A. Dines, B.A. Of two theories which have been suggested to account for the formation of storms—(1) the convection theory, that the central air rises in consequence of its greater relative warmth produced by the latent heat set free by condensation; (2) the theory that storms are circular eddies produced by the general motion of the atmosphere as a whole—the author prefers the convection theory, but he thinks that more information about the temperature of the upper air is greatly needed.

2. "On Brocken Spectres in a London Fog," by Mr. A. W. Clayden, M.A. During the dense fogs in February last, the author made a number of experiments with the view of raising his own "spectre." This he ultimately succeeded in accomplishing by placing a steady limelight a few feet behind his head; measurements and photographs of the spectre were secured.

3. "An Account of the 'Leste,' or Hot Wind of Madeira," by Dr. H. Coupland Taylor.

Mr. Shelford Bidwell, M.A., F.R.S., showed the effect of an electrical discharge upon the condensation of steam. The shadow of a small jet of steam cast upon a white wall is, under ordinary conditions, of feeble intensity and of a neutral tint; if the steam is electrified, the density of the shadow is at once greatly increased and it assumes a peculiar orange-brown hue. The electrical discharge appears to promote coalescence of the exceedingly minute particles of water contained in the jet, thus forming drops large enough to obstruct the more refrangible rays of light. It is suggested that this experiment may help to explain the intense darkness, often tempered by a lurid yellow glow, which is characteristic of thunder-clouds.

Notes on Visits to some American Observatories.—III.

[Continued from p. 192.]

For greater facility in verifying variability, Prof. Pickering has devised and completed the preliminary tests of a new photographic photometric method, which promises to give excellent results on the wholesale scale by the use of a doublet. A small prism of *very* small angle is attached to a photographic doublet of short focal length and mounted equatoreally. The prism covers a very small fraction of the aperture of the doublet, and is intended to produce a small deviation with as little dispersion as may be. A photographic plate is set in position, and the arrangement is pointed to a star. In place of a single image of the star, two photographic images are got, separated by an interval depending on the focal length of the doublet and the deviating power of the prism—one small image due to the prism-covered part of the aperture, and one large image due to the uncovered part. Suppose stars A_0, B_0, C_0 to be photographed—each main image, A, B, C, is accompanied by a companion *a*, *b*, *c*. It is easy to distinguish artificial companions from main images of other faint stars, and comparison may be instituted between, let us say, *a* and B and C, which from the nature of the stars may be a much more advantageous comparison for the detection of variability than a comparison of A with B and C. For instance, suppose the relative brightnesses are represented by 990, 9, and 11, and that the relation between main image and companion is 99 : 1, we have then on the plate what we may represent thus :

990

9 10 **11**

·09 ·11

the dark figures denoting main images, the light ones, to the south-east in each case, denoting the artificial companion. And it is clear that for detection of variability a comparison of plates containing **9**, 10, and **11** at different epochs is better than one between plates containing only **990**, **9**, and **11**, on account of the nature of the law connecting size of photographic image with star magnitude and exposure. One has all needful information for a given epoch on a given plate, and for this reason many objections that would present themselves at first sight, such as difference of diffraction pattern, and dispersion, seem to disappear. Here there is work for a cohort to attack, and who knows but that there is in store for it an incidental romance like that of the discovery of the duplicity (I do not refer to any *moral* obliquity) of β Aurigæ? To see the originals of many of the photographs of this double and of many other interesting spectra, such as that of η Argûs, was another of the treats with which Prof. Pickering regaled me; but I must not dwell on the interesting points still needing further elucidation in the case of the β Aurigæ photograph. Nor must I attempt to warm my readers by an account of Niagara, which we visited after leaving Boston, and found in all the majesty of perfect winter plumage. Whilst at Boston I made a day's excursion to Worcester, where Prof. R. Michelson, of the Clark University, most kindly showed me his beautiful experiments, in which he makes optical interference phenomena a powerful weapon wherewith to attack various questions of molecular physics and astronomical measurement.

In vol. xxxix. of the Amer. Journ. Sci. (1890), and in the 'Philosophical Magazine,' vols. xxx. (1890) and xxxi. (1891, April number), are three papers by Prof. Michelson on the subject, and they will be found to repay careful study. In the second paper he writes :—"The simplest way of effecting this [the conversion of a telescope into a refractometer and consequent increase of accuracy ten or even fifty fold] is to provide the cap of the objective with two slits adjustable in width and distance apart. If such a combination be focussed on a star, then, instead of an image of the star, there will be a series of coloured interference-bands with white centre, the bands being arranged at equal distances apart and parallel to the two slits. The position of the central white fringe can be marked from ten to fifty times as accurately as can the centre of the telescopic image of the star. One of the most promising applications of the method is the measurement of the angular magnitudes of small sources of light." If the angular magnitude as seen from the objective is equal to

$$\frac{\text{wave-length of source considered homogeneous}}{\text{separation of the slits}}$$

the fringes disappear, according to the usual statement. With a given source, as the separation of the slits increases from nothing, the fringes pass through maxima and minima of distinctness—the result of the fact that in the focal plane of the objective we have to deal with the effect of the superposition of the interference patterns due to each element of the source, account being taken of the distribution of light on the source.

Before we can measure the diameter of a source a knowledge of the distribution of light over it is necessary. But the mere existence of maxima and minima of distinctness which might be detected in observing a star would at any rate show that the star subtended an angle greater than that subtended by the wave-length of light at a distance less than the diameter of the objective. At Harvard experiments are being undertaken at Prof. Michelson's request to test the question.

Allegheny was the next point to which astronomical interests took me, and here I spent a few hours enjoying the kindness of

Mr. Brashear, in whose workshops I saw great activity and some beautiful optical work. He has on the stocks several spectroscopes for large instruments, notably one for Prof. Young, who demands all imaginable conveniences suggested by his experience; whilst Mr. Brashear realizes them with all the zest of an enthusiast.

Mr. Very kindly showed me the observatory where so much of Prof. Langley's work was done: one feels there that the bird is flown, and it is to be hoped that at the Smithsonian Institute at Washington the press of executive work, which falls on Prof. Langley as director, will not completely absorb energy that could so well find other outlets. The wooden buildings behind the Institute already contain many engines of war, notably the splendid heliostat of Grubb's construction and the stand for the bolometer.

Leaving Allegheny early in the morning, we travelled over the very finely engineered line crossing the Allegheny Mountains, and reached Princeton in the evening. Prof. Young had not hesitated to put off an engagement on my account, a kindness for which I am still conscience-stricken but very grateful. We spent some time in the Halstead Observatory; it was again my good fortune to have a very fair observing night, and we turned the 23″ on to the Orion Nebula, to Aldebaran and his companions, Neptune and his satellite, 32 Orionis, and some other objects, more than enough to give one an idea of the excellence of the glass. The lenses of the objective are separated by a considerable interval, the flint being of appreciably smaller aperture than the crown, the combination having an effective aperture of 23″. The spectroscope, when not in use, is kept in a sliding framework, by means of which the attachment to the telescope is facilitated, a matter of considerable importance when the telescope is used for varied purposes.

From Princeton we travelled with Prof. Young to New York, enjoying much the opportunity of some talk on astronomical questions. In speaking of solar observations and the difficulties arising from unequal heating of the air in the tube of the telescope, about which I had asked him, Prof. Young mentioned the interesting fact that he had frequently noticed that on throwing the telescope from one side of the pillar to the other the observing was bad for ten or fifteen minutes. Lord Rayleigh has dealt with the disturbance to definition arising from temperature differences, and calculates that a difference of 1° C. in a stratum of air occupying a moderate fraction of the total volume in a horizontal tube will produce a not insensible effect on definition when the length of the tube and stratum is 12 centimetres; the difference of temperature producing a given disturbance varying inversely with the length, difficulties are magnified in large instruments.

Our plans led us again to pass through New York without staying there, and this time passed by day, plunging into the stream of humanity that pours up the stairs leading to the overhead railways; and so we reached Newhaven.

We found Yale in a state of festivity, which reminded us of our

May week, not least in the matter of weather. Here we spent a couple of days, and Dr. Elkin showed me the heliometer with which he has made his well-known measurements for the determination of stellar parallax. The weather had changed and the thaw set in with storms of rain and steaming moisture, and I still think with some disquietude of the dripping condition of that fine instrument which Dr. Elkin has embellished with so many convenient devices of lighting and automatic reading, in addition to those with which it abounded. This is, I believe, the only heliometer at present in the United States. The splendid use which Dr. Elkin has made of it makes one wish that he would devise some method of obtaining wholesale measurements; but I think that even he would doubt the feasibility of a heliometer-doublet. Prof. Newton, with whom Dr. Elkin made me acquainted, extends his investigations of meteorites in all directions, even into mediæval paintings, for he has studied the Fireball in Raphael's "Madonna di Foligno," and into remoter antiquity, for he has his eye upon the huge stones in Greek temples supposed to have been of meteoric origin; and his collections of small meteorites—one lot of nearly three hundred collected from a very small area in Dacota—are peculiarly interesting.

I have passed the bounds that I had imposed upon myself for my notes, and I must hesitate before giving my Hungry Editor an indigestion—the food has been too simple for any fear of gout; but I cannot conclude without saying that, though I hope I have pictured something of the kind of interest and pleasure we have derived from our visit to America, I can do little to convey any idea of the many memories of kindest hospitality and of courteous thoughtfulness in helping a hurrying traveller to attain the object of his visit.

H. F. NEWALL.

"Smithsonian" Astro-Physical Observatory.

THE Smithsonian Institution has established as one of its departments a Physical Observatory which, with the instruments, has been supplied from the Smithsonian fund. It occupies at present a temporary structure, though funds have been subscribed for a permanent building when Congress shall provide a suitable site. For the maintenance of the Observatory an appropriation has been made by Congress which will become available on the 1st of July 1891. The actual instrumental work of the new Observatory will necessarily devolve largely upon a Senior and a Junior Assistant, who have not yet been appointed, who can devote their entire time to research, and it is hoped that with the improved apparatus it will be possible to prosecute advantageously investigations in Telluric and Astro-physics, and particularly those with the bolometer in radiant energy.

In accepting the position of Assistant Secretary of the Smithsonian Institution in 1887, Mr. Langley retained the Directorship of the Observatory at Allegheny for the purpose of completing the researches begun there; and after his appointment as Secretary of the Institution, he still continued the titular Directorship, though but a limited amount of time could be spared from his official duties at the Capital. With the completion of the equipment of the little Observatory at Washington, he, however, formally resigned, on April 30th, 1891, the Directorship of Allegheny which he had held since 1867, and he will, so far as his administrative occupations permit, give personal attention to the general directions of the investigations.

The class of work which is referred to does not ordinarily involve the use of the telescope, and that which is contemplated is quite distinct from what is carried on at present at any other Observatory in the United States. The work for which the older Government Observatories at Greenwich, Paris, Berlin, and Washington were founded, and in which they are now chiefly engaged, is the determination of relative positions of heavenly bodies and our own place with reference to them. Within the past twenty years all these Governments, except that of the United States, have established Astro-Physical Observatories, as they are called, which are, as is well known, engaged in the study of the heavenly bodies as distinct from their positions; in determining, for instance, not where, but what the Sun is, how it affects terrestrial climate and life, and how it may best be studied for the purposes of the meteorologist, and for other uses of an immediately practical nature.

The new Observatory is established for similar purposes. Its outfit includes a very large Siderostat (recently completed by Grubb), which is mounted in such a way as to throw a beam of light horizontally in the meridian. It is intended to carry a mirror of 20 inches diameter, and is, perhaps, the most massive and powerful instrument of its kind ever constructed. Within the dark room is mounted another large instrument, the spectrobolometer, which is, in effect, a large spectroscope with 20-inch circle reading to 5″, specially designed for use with the bolometer. It was made by William Grunow & Son, of New York, as the outcome of Mr. Langley's experience with smaller apparatus during his earlier investigations. The most important part of the instrumental equipment is completed by specially designed galvanometer-scales and a peculiar resistance-box; and these three instruments used in conjunction with the bolometer, and perhaps with the aid of photography, will be employed in the investigations upon light, heat, and radiant energy in general, for which the Observatory is primarily intended, though some departments of terrestrial physics may also receive attention.

A Papal Brief on Astronomy.

In the interesting editorial article in the May number of the 'Observatory' entitled "The Astrophotographic Chart," reference was made to the munificence of His Holiness Leo XIII. in founding an observatory at the Vatican, and endowing it especially as one of the associated observatories for the furtherance of the work of the photographic map. The document by which the Holy Father signifies his good pleasure in this regard is sufficiently important to merit a more detailed notice. It is proposed therefore to give a digest of the brief, adding a few explanatory notes, and translating the passages of greatest moment. The brief bears the title "Of the Re-establishment and Extension of the Vatican Observatory." It opens by rebutting the charge, sometimes made by the ignorant and ill-affected, that the Catholic Church is an enemy to the progress of science. The Supreme Pontiff recalls the services of the Church not only as regards the sacred sciences, but also in the matter of the philosophical, moral, and social sciences. "Nor has She been neglectful of those sciences which have as their aim the study of nature and its forces." This She has done by founding schools and museums, and by rewarding and encouraging such of Her children as have been distinguished in their pursuit. His Holiness proceeds:—"In the first rank of these sciences shines astronomy, the object of its investigations being the stars, which, more than any other inanimate objects, recount the glory of God .... Now the Sovereign Pontiffs have an especial motive for favouring the progress of this science and for encouraging those who devote thenselves to it, in the fact that by its means alone can those days be determined with certainty which have been set aside for the celebration of the greatest and most religious ceremonies of the mysteries of Jesus Christ."

The next few paragraphs are historical and contain some interesting details. They record that, in order to carry out the reform of the Calendar insisted upon by the Council of Trent, Pope Gregory XIII. constructed a tower in the Vatican to be used as an observatory. In this tower a round marble table was erected, upon which Ignatius Danti of Perugia drew a meridian line. The table was likewise engraved with astronomical signs, and served as a sun-dial. The tower having fallen into disuse was under the pontificate of Pius VI. at the end of the last century restored to its original purpose. "Then by the zealous care of Philip Gili, a Roman prelate, other observing-rooms were added for researches on the magnetic current, the weather, and the life of plants." This observer dying in 1821, the observatory of the Gregorian Tower was for the second time alienated from its proper purpose. But Leo XII. in his Apostolical Letters entitled "*Quod divina sapientia*," again made dispositions concerning the taking and the publication of astronomical observations. Other sites, however, were preferred to the Vatican, on account of the horizon of the latter place being impeded by the dome of St. Peter's. The reference is doubtless to the restoration of the Roman College and its observatory to the Fathers of the Society of Jesus in 1824. This observatory, in which Clavius observed Tycho's star in Cassiopeia in 1572, had been the scene of the labours among others of the Fathers Scheiner, de Gottingues, Asclepi, Borgondi, Maire, and Boscovich. After its restoration it was directed by Fathers Dumouchel, Di Vico, and Secchi. On the death of the last-named in 1878, it was together with its instruments, the gifts of Popes, Generals of the Society, and other generous benefactors, appropriated by the Italian Government. It was their good fortune to find such a distinguished observer as Signor Tacchini to carry on the work of the institution. Leo XII. also added to the University of Rome the observatory of the Capitol, and he entrusted it to the charge of J. Calandrelli. Pius IX. appointed Respighi as its director, who held the post until his death.

But to return to the present brief. The Pope proceeds :—"But after these places with the rest of the city had fallen into the power of the stranger, We received, amongst many other presents on the occasion of the fiftieth anniversary of our priesthood, several instruments of perfect construction for the use of learned men who are devoted to celestial, atmospheric, and terrestrial observations. Men most experienced in physical sciences considered that there was no place more fitted for their reception than that very same which Gregory XIII. seemed to have prepared for them in the palace of the Vatican." In the next paragraph he states that he has given orders that the observatory should be "provided and furnished with all things necessary, not only for astronomical studies, but likewise for physical researches, and for the knowledge of atmospheric phenomena." In order the better to secure a free horizon he commands that an annexe to the observatory should be established in the Leonine Fort which surmounts the Vatican hill.

"We have given orders for the installation in this place of the large optical instrument which is called an equatorial, and which is constructed for the purpose of receiving the photographic images of the stars. To this end, We have chosen competent and experienced men, capable of carrying out all that is required for the accomplishment of this enterprise, and We have placed at their head a person well versed in matters astronomical and physical, Father Francis Denza, of the clerks regular of St. Paul, called Barnabites. Trusting to their skill, We have willingly consented that the observatory of the Vatican should be associated to, and should enter into joint labours with, the other distinguished institutions established for the progress of astronomical science, which have been charged with the care of drawing up photographic tables, which will permit of the construction of a perfect picture of the entire heavens with the multitude of stars which cover it."

The next paragraph states that in order that the work may be of a permanent character a committee has been chosen, and a constitution drawn up for their guidance, which are "by these presents solemnly confirmed, and it is Our wish that this establishment should hold the same rank as the other Pontificial institutions set aside for the cultivation of science." Moreover, a capital sum is allotted the interest on which will suffice for its maintenance and preservation in a befitting manner. "But for its prosperity in the future We rely more on the favour and help of Almighty God than upon human precautions; for in this undertaking We have in view not only the progress of a noble science, which more than any other human study raises the mind of man to the contemplation of heavenly things; but We have above all set before Ourselves that which has ever been from the beginning (of Our Pontificate) the object of Our concern and the aim of Our efforts, to show to all men, on every occasion that presents itself, both by Our words, by Our writings, and by Our deeds, that the Church and Her pastors, as We have said at the beginning (of this brief), far from hating true and solid science, as well in things human as in things divine, favour, protect, and actively help its progress with all their power."

"Wherefore We will that whatsoever We have ordained and decreed by these present Letters shall remain ratified and confirmed in the future, as they are at present, and We declare null and void all that may be done to the contrary, by anyone whatsoever. And this all to the contrary notwithstanding.

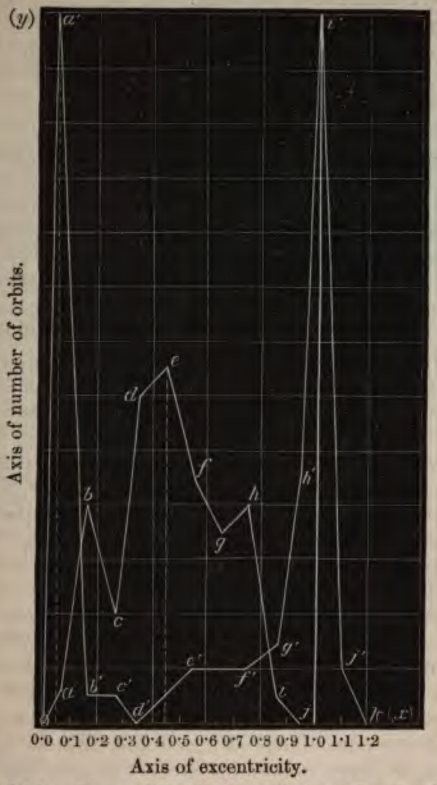
"Given at St. Peter's in Rome, the 14th day of March 1891, the year XIV. of Our Pontificate.—Leo XIII. Pope."

It will have been noticed how anxious the Holy Father is that the present observatory should be established on a firm and permanent basis. To ensure this still more he has affixed to the title of the present letter the clause "motu proprio." A few remarks on the force of these words when they occur in the superscription of a Papal brief may not be out of place here. Their technical meaning in a court of canon law is that the Pope was not moved in the granting of a favour, for example a benefice, by the instances or petitions of any person, even in the case when such instances preceded the issue of the brief, but that it emanates from his own spontaneous action. The force of the additive clause is this, that the brief which bears it cannot as a rule, for there are three exceptions not necessary to specify here, be impugned in the law courts on the score of "subreption," which is a concealing of the truth, or the omission of any formality required to be expressed therein. It could, however, be quashed on the score of "obreption" should it set forth what is evidently false. We can only conclude with the wish that the hopes of the Holy Father may not be frustrated, and that the observatory of the Vatican founded by his munificence may enter on a long and prosperous career.

A. L. Cortie.

Comparative Excentricities Illustrated Graphically.

If we construct a diagram in which the axis of x is taken as the "axis of excentricity," and that of y as the "axis of number of orbits," we may illustrate graphically the distribution of orbits as respects the different regions of excentricity. In the accompaning figure such a representation is made for the binary, planetary, and cometary orbits. The binary curve is constructed from a table of 63 orbits—all that have been computed. The mean excentricity is 0·45, and the diagram shows that the orbits are symmetrically distributed, and nearly in a probability curve.



$0\,a\,b\,c\,d\,e\,f\,g\,h\,i\,j$ = Curve of distribution of the orbits of stellar systems.

$0\,a'\,b'\,c'\,d'$ = Curve of distribution for the planetary system.

$d'\,e'\,f'\,g'\,h'\,i'\,j'\,k$ = Curve of cometary orbital distribution.

The orbits of the great planets and their satellites are nearly circular, the mean excentricity being less than 0·0389, or about *one twelfth* of the binary mean.

The cometary orbits, on the other hand, are nearly parabolic. Owing to the great number of cometary orbits the parabolic ordinate of the curve is drawn on a somewhat reduced scale; and as the hyperbolic orbits are somewhat doubtful, only the probable nature of that branch of the curve has been indicated.

Accordingly, in the diagram we see at a glance the curves of distribution (as respects excentricity) of the *three great classes* or *species* of cosmical orbits. This graphical illustration may be regarded as an emphatic indication that the stellar systems are the products of a general process of nebular evolution, and not the results of fortuitous approximations, as are the Comets which have entered the sphere of attraction of the Sun. T. J. J. SEE.

University, Berlin, 1891, May 18.

Selenographical Notes.

JANSSEN.—This large irregular walled-plain is familiar to most telescopists mainly on account of the prominent rill-like valley traversing it from north to south, a feature which, under a low Sun, may be seen with as small an aperture as 2 inches, while with greater magnification it is one of the most noteworthy objects of its class on the lunar surface. The observer whose scrutiny of the formation has been principally confined to an occasional view of this curious valley, is aghast when he turns to Schmidt's Chart, Sect. 24, and sees the intricate detail shown both within this great enclosure and in its neighbourhood, contrasting in this respect so strongly with Mädler's representation, and still more so with that of Neison. There are possibly still some who may be inclined to question whether in this and other instances the crowd of craterlets large and small, the great assemblage of ridges, hills, and other minuter features, all have an actnal existence, especially as not a tithe of them can, as a rule, be traced on any one given night, even under the best conditions. When the map was published, it must be confessed that I was among the doubters; but subsequent experience has tended year by year to increase one's admiration of this noble achievement, and to show that this scepticism was due to the lack of that well-nigh exhaustive acquaintance with most of the lunar formations which Schmidt had acquired during long years of patient observation. It must, nevertheless, be admitted that with all its marvellous accuracy and fullness of detail, it is very unequal, displays some most unaccountable omissions, and is usually far from satisfactory in the portrayal of ridges and central mountains. As regards the latter features, however, it is pretty certain that its imperfections must be attributed to a coarse style of reproduction, for which the author was not responsible. To return to the drawing of Janssen: Schmidt shows the great rill-like valley as commencing at a deep hole or depression under the south wall of Fabricius, and following, with its width undiminished a slightly curved course (the convexity being turned towards the east) to a group of hills situated about midway between this formation and the southern limits of the enclosure. South of the central hills the valley, becoming much narrower, seems to differ in no essential particular from a true rill. At a point a little south of the central hills a second cleft (Neison's ϕ) branches from the west side of the valley and runs up to the foot of the south-west border of Janssen, where it terminates at a small deep pit. On April 13, 1891, 8^h to 9^h, these features were well seen, and, in addition, two other distinct clefts not shown in the maps. The first, commencing at an isolated mountain a little east of the point of junction of φ with the great valley, runs nearly parallel to the latter up to the foot of the southern wall of Janssen; the second, originating at this end of the first, runs for some distance at the foot of the border, passes the southern termination of the great valley, and then, running obliquely up the inner slope of the wall, terminates about midway between the top and bottom of this declivity at a bright oval mountain. These clefts were steadily seen with powers 284 and 350 on my 8½-in. Calver, and were traced nearly as distinctly, under a much higher angle of illumination, on April 15, 8^h 30^m. On the east side of Fabricius, in the rugged region between this formation and the border of Janssen, are two curious long linear markings which cross each other near their eastern extremities. One appears to agree as regards position with part of a narrow valley drawn by Schmidt, the other is not shown. Both seem to be coarse clefts or very narrow depressions between parallel longitudinal ridges. Only a central peak is shown in the interior of Lockyer by Mädler and Neison, while Schmidt draws a peak on the north-east side of the floor and two small craters on the opposite side. On April 15th this peak was obvious, and in addition, a crater, much larger than either of those shown by Schmidt, was noted on the floor at the foot of the south-west wall. No trace of the other could be detected. Careful sketches are much needed of that very remarkable formation Fabricius, and its companions on the north and west, Metius and Fabricius *a*. The first, in addition to a somewhat complicated terraced western border, has an inner concentric wall like Taruntius, the eastern section of which appears at certain phases of illumination to be associated with the eastern wall of Metius.

SELENOGRAPHICAL STUDIES BY M. VICTOR NIELSEN OF COPENHAGEN.—Seven carefully executed drawings, accompanied by descriptive notes, have been received from this observer relating, 1 and 2, to Agrippa; No. 3, to Plinius; No. 4, to Hyginus; and Nos. 5, 6, and 7 to the Triesnecker rill-system. They have been well reproduced in the March number of the 'Journal of the British Astronomical Association.' With respect to the dark line noted as crossing the wall-enclosed plain west of Agrippa, it is probably either a fault or the shadow of a ridge and not a cleft.

I have observed it many times, but have never been able to see any indications of an opening in the surface. The two crater-like depressions on the central mountain of Plinius, observed by Prof. Thury and subsequently by M. Nielsen, were seen simultaneously by M. Gaudibert and myself on January 25, 1882, and a sketch of the formation from my drawing published in the 'English Mechanic' the following week. The curious depression in the inner slope of the eastern wall of Plinius is well shown by M. Nielsen, as is also the crater on the south border. Two craterlets, on the northern quarter of the floor, usually conspicuous under a low Sun, seem to have escaped notice. The white central point within Hyginus, observed 1890, Jan. 28, 7^h 15^m, doubtless represents part of the raised border of the cleft which traverses the floor. M. Nielsen's observations were made with a 6½-inch achromatic by Reinfelder and Hertel of Munich.

Beaumont House, Shakespeare Road, THOS. GWYN ELGER.
Bedford, 1891, May 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

La Carte du Ciel.

MESSIEURS LES ÉDITEURS,—

La notice que vous venez de publier dans l'*Observatory*, au sujet de ma proposition à la dernière réunion du Comité de la Carte du Ciel, sur l'ordre dans lequel on pourrait faire les deux séries des clichés, semble faire supposer que j'attache plus d'importance au catalogue qu'à la carte du ciel.

Quand vous aurez lu les Procès-verbaux des Séances, malheureusement un peu retardés par la nécessité d'attendre les épreuves corrigées de l'étranger, vous verrez que c'est une idée toute contraire qui a dicté ma proposition. Attachant beaucoup plus d'importance à la carte du ciel qu'au catalogue, parce qu'elle contiendra trois grandeurs d'étoiles de plus, je proposais de la faire séparément avec tout le soin qu'elle exigera, après qu'on se serait débarrassé du catalogue en un an ou deux de travail, et que les astronomes auraient acquis une grande expérience des procédés de photographie stellaire en faisant cette première série, plus facile à obtenir d'ailleurs que la seconde; de plus on pouvait espérer que ce retard permettrait peut-être d'utiliser encore quelque nouveau progrès dans les procédés photographiques.

Le temps total exigé pour l'exécution des 2 séries de clichés sera à peu près le même qu'on les fasse séparément ou ensemble. On ne retardait donc nullement la terminaison de la carte du ciel en la faisant après le catalogue ; seulement on aurait pu alors la faire avec plus de soin et de succès. Mais, dès que j'ai compris que cela soulevait quelques doutes et que l'on pouvait supposer compromise l'exécution de la carte du ciel, je me suis empressé de retirer ma proposition.

Je puis bien vous avouer d'ailleurs que j'ai été quelque peu contrarié, au congrès de 1887, quand j'ai vu émettre le vœu, bien inattendu pour moi, de faire 2 séries de clichés ; je craignais de voir compromettre ainsi la seconde série de la carte du ciel, qui dans ma pensée devait pouvoir satisfaire à tous les besoins, même d'un catalogue ; et ce n'est que sur l'avis des astronomes les plus compétents que j'ai voté en faveur de cette nouvelle série, pensant qu'il y avait grand intérêt à ce que toutes nos décisions fussent prises à l'unanimité.

Votre bien dévoué,
Amiral E. Mouchez.

Observatoire de Paris,
13 mai, 1891.

The Radiant of the August Perseids.

Gentlemen,—

My observations of the Perseids and of the displacement in their radiant-point were made entirely free from theoretical bias, for at the time the meteors were recorded and their centres of radiation determined, I gave the matter no thought as to the direction or the rate in which the radiant should be moving. The facts observed were noted apart from any considerations as to the requirements of theory.

Let us now see how the data obtained in this independent manner agree with mathematical views. Mr. J. Kleiber, of the Imperial Observatory, St. Petersburg, has just written a large and elaborate work "On the Determination of Orbits of Meteor-streams," and on page 328 he says if a meteor-shower constitutes a ring (ellipse) having a definite orbit in space then the elements i and $\pi -$ ☊ are constant for all parts of the ring. "This gives a very simple and elegant rule for the displacement of the radiant-point of a meteoric ring, viz. the latitude of the apparent radiant remains constant; longitude increases proportionately to time, but the quantities ⊙ − long. and lat. are constant for every radiant."

The radiants I derived from Perseid showers during the long activity of that stream precisely fulfil the conditions required. Mr. Kleiber himself is led to remark that his computations "confirm the shifting of the radiant of the Perseids during the six weeks of the activity of the shower as observed by Mr. Denning. A glance at the orbits corresponding to all the radiants related by Mr. Denning to the Perseid shower will show their identity at once" ('Monthly Notices,' April 1891, p. 407).

The virtual coincidence of my adopted Perseid radiants with the direction and rate of motion which the wide elliptical stream of Perseids must really show can only be referred to actual identity of the phenomena. It is satisfactory to note the agreement and to have this assurance of the accuracy of observation. More evidence is certainly required as to the character and position of the display during the second week in July and third week in August. In the present year moonlight will partly interfere at these times, but from about July 28 to August 14 the shower may be watched during the interval of its greatest activity and the observer will do well to note the displacement of the radiant from night to night. He will probably find evidence of several minor showers in Cassiopeia near δ and ζ, and in Perseus near α–β, ϵ and μ, but the great stream of Perseids has an individual character due to the remarkable abundance of its meteors and to the easterly motion of the radiant-point.

Yours faithfully,
Bristol, 1891, May 15. W. F. DENNING.

Mr. Stockwell and Historical Eclipses.

GENTLEMEN,—

In No. 240 of the 'Astronomical Journal,' Mr. Stockwell returns to the subject of historical eclipses. I have nothing to add to the remarks which you have already kindly inserted from me with regard to those which took place before the Christian era, except that it appears to me that the authors of 'L'Art de vérifier les Dates' have satisfactorily proved from Censorinus that the first year of the first Olympiad corresponded with B.C. 776, so that in later times the years in which the Olympic festival was observed preceded the bissextile years of the Julian calendar. It is well known (and this may account for the passage in Julius Africanus referred to by Mr. Stockwell) that a mistake was made after Cæsar's reformation of the calender in the observance of his rule, which was rectified by Augustus 36 years after its establishment. There seems no doubt that Cæsar arranged that the first year after his reformation (when the Julian style came into force) should have the intercalary day or be a leap-year, that year corresponding to what we call B.C. 45, the year after which Cæsar was assassinated.

But the principal point to which on the present occasion I wish to allude is Mr. Stockwell's contention that, chiefly on the ground of some recorded eclipses, it is necessary to alter the received dates of Roman history for a considerable time before and after the Christian era, and take the events as having happened one year earlier than is generally supposed in our chronology. I will show *seriatim* why I cannot accept his view:—

1. Mr. Stockwell contends that the death of Augustus occurred in A.D. 13, not 14, because an eclipse of the Moon is recorded by Tacitus to have taken place during an insurrection shortly afterwards of the troops in Pannonia, and he thinks this must have been the eclipse of October 7, A.D. 13, rather than of September 27, A.D. 14, chiefly because the latter took place in the second and the former in the first half of the night. I must confess the passage in Tacitus seems to me (as apparently it also did to Dean Merivale) to be most consistent with an eclipse the greatest darkness

of which was late in the night or even towards morning; and when we also consider that the eclipse of October 7, A.D. 13, was a small partial, and that of September 27, A.D. 14, a total one, the latter would seem far more probably to be the one alluded to.

2. Mr. Stockwell adduces two eclipses (one of the Sun, the other of the Moon) related to have occurred within fifteen days of each other early in the reign of Vespasian. They are recorded, he says, by Pliny the Younger; but this is a *lapsus plumæ* for Pliny the Elder, as the account is in his 'Nat. Hist.' ii. 10. That Mr. Stockwell is right in considering the two eclipses to be those of the Moon on March 4 and of the Sun on March 20, in the year A.D. 71, I have no doubt whatever. But it is impossible to conclude that this was in the third year of Vespasian's reign. Pliny gives the consulates, and there is much doubt as to what the true reading of the passage is. If he means the third *consulate* of Vespasian (as seems most likely), that would be the year A.D. 71 and the first year of the emperor's reign. There seems therefore to be no ground whatever here for altering the received date.

3. Finally, Mr. Stockwell brings forward another argument not derived from eclipses, in which he has apparently fallen into an inadvertence. When Cæsar was making his last campaign in Spain against the sons of Pompey, a battle was fought at Soritia on the third day before the nones of March (*i. e.* on Mar. 5). Cæsar immediately afterwards pursued the enemy and commenced his march, Mr. Stockwell says, before the Moon rose about midnight. Now as in B.C. 45 (the received date of this event) the Moon was New on March 1, she could not have reached her first quarter on March 5, and must have risen in the daytime; hence he proposes to alter the date to B.C. 46, when the Moon would have been New on Mar. 12. But besides that Mr. Stockwell appears here to have forgotten the confusion in the Roman calendar at the epoch in question. He seems also to have misunderstood the unknown author of the book 'De Bello Hispaniensi' *in loco*, c. 27. For the passage runs thus:—"Cæsar priusquem eodem est profectus, luna horâ circiter VI visa est." The writer thought it a remarkable thing that the Moon was seen distinctly in the middle of the day, and the circumstance shows that she was in about her first quarter at the time.

Yours faithfully,

Blackheath, 1891, May 8. W. T. LYNN.

P.S.—Since the above was written, another number of the 'Astronomical Journal' has appeared, in which Mr. Stockwell has a short paper on the eclipse of the Moon on the night preceding the battle of Pydna. He contends that this event occurred four years before the dâte (B.C. 168) usually accepted for it, on the strength of a lunar eclipse which took place on September 3, B.C. 172. But the account in Livy (lib. xliv.) shows clearly that the eclipse on the eve of the battle occurred in the early part of the night, which was not the case with that just mentioned; and there can be little doubt that the ordinary date is correct and the eclipse that of June 21, B.C. 168.—W. T. L.

Signs—and Wonders.

GENTLEMEN,—

For the *n*th time I would enter my protest against the misuse of the symbol ″ for inches, of which Mr. Newall is guilty in his very interesting "Notes on Visits to some American Observatories," in your number for this month. The signs °, ′, and ″ have a perfectly definite signification and equally definite connotation; and it can only tend to a slipshod method of thought and to inaccuracy, to attach any other meaning whatever to them than that of degrees, minutes, and seconds of *arc*. ″ ought not even to be employed to signify seconds of time. If there be any excuse for a carpenter making ″ stand for inches (which is, to say the least, doubtful), assuredly there can be none for an astronomer doing so. It would be just as legitimate to employ *£*, *s*, and *d* to stand for hundredweights, quarters, and pounds.

Faithfully yours,
WILLIAM NOBLE.

Forest Lodge, Maresfield,
Uckfield, 1891, May 4.

The Red Spot on Jupiter.

GENTLEMEN,—

Observers of Jupiter will be pleased to learn that the old red spot has now regained much of the prominence and depth of colouring which it possessed in 1879 and subsequent years, and which caused it to form at that time such a splendid and attractive feature. I have observed it on several occasions lately with my 6½-in. Calver reflector, and it has always appeared very conspicuous and striking, even when seen under unfavourable conditions. The following transits have been observed here:—

1891, May 2, $16^{\rm h}$ $28^{\rm m}\cdot5$ G.M.T.
14, 16 23 ·5

So that the spot may be expected on the central meridian of the planet's disc about $4^{\rm m}$ after the times given in Mr. Marth's Ephemeris in the 'Monthly Notices.'

It is perhaps worth noting that the present increased plainness of the spot occurs about 12 years (or the period of one revolution of the planet round the Sun) after it first attracted universal notice in 1879.

Yours faithfully,
A. STANLEY WILLIAMS.

Burgess Hill, 1891, May 21.

Observing Occultations.

GENTLEMEN,—

I was much interested in Capt. Noble's letter in the May number of the 'Observatory,' as the device, which he there describes as useful in the observation of occultations, is one which I have been accustomed to employ, in a somewhat modified form, with very great success. It has been my custom to place the star, before immersion, near the intersection of the thread placed parallel with the equator, with the thread at right angles thereto, or, so to speak, in the angle formed by the thread. If the instrument is in good adjustment and the driving clock running fair, one is then enabled to know, not only the position on the Moon's following limb at which the emersion will take place, but also, by noticing the position of the angle of the thread with reference to the limb, the time at which it is necessary to look out for the emersion. This very simple expedient does away with the necessity of those tiresome calculations which the observation of emersion is supposed to entail, and if generally followed would, I think, have a most beneficial result in reducing the great disproportion which now exists between the number of observations of immersions and emersions.

Yours faithfully,
JOHN TATLOCK, Jun.

New York, U.S.A.,
1891, May 19.

OBSERVATORIES.

WINDSOR, N. S. W.—We are pleased to receive Mr. Tebbutt's Report on work done in 1890, from which it appears that his instrumental equipment is the same as last year. The observations with the Transit were chiefly for time-determination. With the 8-inch equatorial 24 disappearances and 3 reappearances of stars occulted by the Moon; 41 phenomena of Jupiter's satellites; 178 comparisons of minor planets; 102 comparisons of comets Denning and D'Arrest; observations of the binaries *p* Eridani, Lal. 4219, γ Centauri, *h* 4634, α Centauri, Brisbane 5784, and γ Coronæ Australis. The observations of the variables η Argûs and R Carini have been continued, and of the former star Mr. Tebbutt says "it has not sensibly varied in lustre since the announcement of its sudden increase of magnitude between April 1887 and May 1888. I propose shortly to publish a careful revision of the Windsor observations from 1854 to 1890." A satisfactory observation of the maximum of R Carini was secured in 1890 April. The meteorological observations were taken regularly at 21^h. A list of presents to the library is appended to the Report. We heartily congratulate Mr. Tebbutt on the splendid work he is doing for science.

LEANDER MCCORMICK.—A catalogue of 171 pages contains the results of a small portion of a series of observations begun at the Cincinnatti observatory in continuation of the Bonn Durchmusterung, and referring to the zone $-23°$. We suppose this zone may be taken as complete, though this is not stated in the few lines of Introduction. The number of stars catalogued is 6671. The instrument used was a 4-in. equatorial by Alvan Clark.

HARVARD.—In the Report for 1890 the Director again refers to the urgent necessity for a fire-proof room to store the accumulated MSS. and photographs. The storage of photographs will require consideration ere long in a large number of our observatories; and it will be well to pay attention to the remarks suggested by the Harvard experiences, notably, that about the great weight of a collection of photographs, which may be a source of danger in buildings not constructed for heavy loads.

The regular work of the observatory has consisted of 21 photometric observations of Jupiter's satellites at eclipses, 67 comet observations, 381 estimations of the 17 circumpolar variables selected for regular observation, and other miscellaneous matters—all with the east equatorial; 6955 observations with the meridian circle on the southern zones −9° 50′ to 14° 10′; and with the west equatorial two investigations on double stars and variable stars respectively. But the energies of the staff seem to have been chiefly directed to publication; and a very creditable result has been attained. The zone catalogue of stars between +55° 10′ and +49° 50′ is all but finished and forms Part II. of Vol. XV. Vol. XIX. Part II. is in type and deals with 50 years' observations of the Zodiacal Light. Vol. XXIII. Part I. has appeared since the report and contains the observations summed up in the "Meridian Photometry," which forms Vol. XXIV. and appeared recently ('Observatory,' No. 172, p. 101). Vol. XXVII., "the Draper Catalogue," has just appeared and is noticed elsewhere; and besides this several meteorological volumes have been published and others are in type. It will take some time to digest all this.

But the vigour of the observatory is shown best in its extraneous developments, such as the Henry Draper Memorial. In Peru 1309 photographs have been taken with the Bache telescope during the year; and at Harvard 2157 with the twin telescope presented by Mrs. Draper. Some of these show simply star-disks and others (obtained with a prism of small angle in front of the object-glass) star-spectra. The plan of the work has been described before (*e.g.* see 'Observatory,' No. 149, p. 223), and the more interesting details of the results are referred to periodically, so that no further mention need be made of them here; but their presentation *en masse* in these annual reports leads one to reflect with admiration on the ability and energy of Mrs. Flemming, Miss Maury, and their assistants.

The investigations of climate with the Boyden fund have proceeded satisfactorily at the three stations, Harvard, Wilson's Peak in California, and Chosica in Peru.

At Harvard much time was devoted to visual observation of Mars: "a number of the so-called canals were recognized, but only one of them was distinctly seen to be double."

"The advantages of the climate of Wilson's Peak are less marked than was anticipated; still they are sufficient to justify an attempt to purchase a suitable site there for a more permanent station."

At Chosica the observers were clouded out and were fain to look for a better station, which was found at Arequipa. The meridian photometry of southern stars is now nearly complete.

The disks of glass for the Bruce photographic telescope had not been received at the date of the Report.

PUBLICATIONS.

Telescopic Work for Starlight Evenings *.—Most people must have noticed a general anxiety at the present time to get information on all sorts of subjects from experts. It would seem to be a development of the system of interviewing that extracts a book about cricket from Dr. Grace, or about spending money from a notorious spendthrift. The idea is simple, and more successful in practice than might have been expected; for experts are notoriously deficient in ability to explain their methods, and it is only occasionally that we find brilliant exceptions to this rule. Mr. Denning is an exception. He has written about his work in a straightforward, earnest fashion, that must not only make many difficulties clear to his readers, but inspire them with something of his own determination and enthusiasm. He shows what work can be done with moderate instrumental means *plus* enthusiasm; and we hope all those who possess the former and are in search of the latter will read his book of 361 pages, in XVII. chapters. Some of the matter has appeared before in this magazine and others, but it has been altered and extended so as to be virtually new. Chapter I. is on the telescope, II. on the relative merits of large and small telescopes, III. and IV. on miscellaneous matters, V. to XIII. on the Sun and planets, XIV. on comets and comet-seeking, XV. on meteors, XVI. on stars, and XVII. on nebulæ and clusters.

We are tempted to give a few of the practical remarks which abound in the book. "A great advantage, both in light and definition, results from the employment of a single lens as eyepiece... By many careful trials I find it possible to glimpse far more detail in planetary markings than with the ordinary eyepiece" (p. 47). "It seems to be a fashionable imposition on the part of opticians to overstate magnifying-powers" (p. 49). "You are right in saying that Mercury is much easier to observe than Venus" (p. 141). "As to the real worth of a glass, there is nothing like having a reliable means of comparison, *i. e.* another telescope of acknowledged excellence with which to test the doubtful instrument." The author might perhaps have added an observer of acknowledged experience to make the test. In dealing with the planets a separate section is always devoted to occultations—a subject specially interesting to observers with small telescopes. For the admirable summary of observations on Mars, we must refer our readers to the book (pp. 160, 161). We wish Mr. Denning had not represented eyepieces placed on the table field-lens down (p. 41): in this position scratching is almost certain.

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* 'Telescopic Work for Starlight Evenings.' By W. F. Denning, F.R.A.S. London: Taylor and Francis.

NEUE ANNALEN DER K. STERNWARTE IN BOGENHAUSEN BEI MÜNCHEN, Band I.—This volume contains a Catalogue of 33,082 stars for the Epoch 1880, deduced from a re-discussion of Lamont's zone catalogues, which were separately published in successive volumes of the 'Supplementbände' to the Munich 'Annalen.' These various star-catalogues have been combined into a homogeneous whole, and the places brought up to 1880 by Prof. Seeliger, the present Director of the Munich Observatory. The stars whose places are given in the catalogue are situated between the limits −32° and +24° declination, and are telescopic stars down to the 10th magnitude inclusive, but are, unfortunately, very unequally distributed over the sky, and the catalogue is therefore not so useful for the purposes of many investigations as it might otherwise have been. But for this, of course, the present editor is not responsible, and he deserves the thanks of working astronomers for having undertaken the enormous labour involved in the preparation of this work, which will doubtless prove one of considerable practical utility. A. M. W. D.

BEOBACHTUNGEN ANGESTELLT AM ASTROPHYSIKALISCHEN OBSERVATORIUM IN O'GYALLA (UNGARN).—This title covers Vols. xi. and xii. of the publications issued from O'Gyalla, the private observatory of Dr. Konkoly, and is a statement of the work done there during the years 1888 and 1889. This work consists of physical observations of the Sun, Jupiter, and comets Sawerthal, Barnard, and Davidson; observations of meteors; and determination of the longitude of Kis Kartel. The minute details recorded of every spot and group observed on the Sun's surface show that this is a special feature of the work. The results of the two years are:—

Year.	Days of observation.	Days free from spots.	Total spots.	Total groups.
1888	252	123	557	162
1889	214	142	208	84

The surface of Jupiter was carefully observed and recorded, and in addition there are 14 lithographs showing Jupiter from 1885 to 1888. The observations of meteors and the account of the longitude operations occupy the remaining space. The observatory is fitted with the electric light, both for use with the instruments and also in the rooms.

NOTES.

The Greenwich Meridian.—The movement for the adoption of the Greenwich Meridian as the standard for Universal Time is making steady progress. One of the last speeches of the late Count von Moltke in the German Reichstag (1891, March 16) was devoted to this subject. The following brief account of this speech, from a Vienna paper, may be of interest.

"Berlin, 16 March. Count Moltke took people greatly by surprise to-day, in the Imperial Diet, by speaking in favour of a single standard time for the Inland Railway service. Although Count Moltke was hoarse, even his longer explanations could be understood throughout the whole room; and the vigour with which they were expressed, and especially the humorous remarks which Moltke introduced, were a reassuring testimony to the health of the ninety-and-one-year-old speaker. The House followed his detailed statements with great attention, and at the same time with lively applause. Moltke wishes for the single time standard chiefly in the military interest. The speaker thought the fifteenth meridian east from Greenwich (the so-called Stargard Meridian) to be most suitable for the time standard. The latter could, within limits, be fixed for a Middle German Standard of time with small changes at the boundaries. A single time for civil life and the abolition of all local times should be striven for as a final aim. The learned men of observatories are only against this, because they do not wish for a German, but for an international standard of time. They could be answered that only a small part of us have business with railways, but a still smaller part with Observatories and their Astronomers, Meteorologists, and Geodesists. [Applause.] Prejudices against the single standard of time in civil life will be easy to overcome. In factories it will only be necessary to alter the figures on the time-bill. Country workers in general do not look at the clock, but only at the clock in the sky; they must then be called by the bell to their work, but when the bell goes wrong, which is the case in general [Laughter], they do not go at the right time to their work. Practical life is not generally exact to the minute; school-clocks are often put back ten minutes, whilst the children wait until the teacher comes [Roars of laughter]; the clock at the court of justice is put back in order that the parties may be there when the trial begins. On railways the clock is generally a few minutes fast; even Parliament has an academical quarter of an hour. [Laughter.] The question cannot be settled by a vote, but the transaction will be made more simple if the Diet expresses itself in a friendly mannner. [Lively applause.] Freiherr v. Stumm replied that the simple peasant and labourer would have no advantage from the introduction of a single standard of time. The subject was then dropped."

We hear also from Dr. Schram, who has written on this subject in our columns, that in the Belgian Chamber the Minister of

Commerce made a speech strongly in favour of the adoption of Greenwich on April 10 last; and that the movement recently initiated in Italy for adopting Jerusalem is being gradually abandoned, the Chamber of Arts and Commerce in Piedmont having on April 9 voted unanimously in favour of Greenwich.

STAR CATALOGUE.—M. Paul Harzer, in the Ast. Nach. No. 3035, gives a useful catalogue of 375 stars between declination ±23° for the epoch 1885·0. They are mostly below the 6th mag., and hence may be found of service as comparison stars in comet observations. The observations were made with the meridian-circle at Gotha.

SOLAR ECLIPSE OF 1893, APRIL 16.—M. Cruls has arranged for securing regular meteorological observations at Ceara (which lies on the line of totality) during April 1891 and 1892. These observations will be published as soon as possible, so as to enable intending observers to judge for themselves the prospects of fine weather.

THE NEW MEROPE NEBULA.—In No. 173, p. 135, we remarked that Mr. Barnard had found by eye-observation a new nebula near Merope, which he considered could not have been discovered by photography, "because a sufficiently long exposure to impress the nebula would so overexpose Merope that its light would coalesce with that of the nebula." Prof. Pritchard replied that this was not the case, as the nebula had already been photographed with comparatively short exposures and was shown quite distinct from Merope; and Mr. Barnard, in acknowledging the truth of this remark (Ast. Nach. 3032), adds that he has examined some plates of the Pleiades taken with the Lick refractor and finds the nebula on all of them.

WE deeply regret the death of Dr. Ed. Schönfeld, Director of the Bonn Observatory, and successor of the illustrious Argelander, whose work he shared and completed. Schönfeld began his astronomical life in 1852 at the age of 24 under Argelander, being by him appointed assistant at the Bonn Observatory in 1854. He made the trial observations for the great Durchmusterung during Argelander's absence at Pulkowa; and was associated with him in the work until called to direct the Mannheim Observatory in 1859. At Mannheim he produced two catalogues of variables and one of nebulæ. In 1875 he was appointed to succeed Argelander, and in loyalty to the traditions of the Bonn Observatory commenced the Southern Durchmusterung, which he accomplished almost single-handed in 10 years, though at the cost of his health and perhaps his life. His services to the University of Mannheim and to the Astronomische Gesellschaft are remembered with deep gratitude. A singularly reserved man, he made no enemies; while his friends were warmly attached to him. He was born on 1828, Dec. 22, and died on 1891, May 1. A melancholy interest attaches to his "Note on the New Star of 1006," which appears in the Astr. Nachr. for May 11 (No. 3034) and the proofs of which were corrected by the author a few days before his death.

THE PARALLAX OF β ORIONIS.—In No. 166 (September, 1890) we printed a note by Dr. Gill on β Orionis in which the parallax of this star was found to be $-0''\cdot064$ with reference to star *a* and $-0''\cdot325$ with reference to star *b*: on which the author remarks: "If these figures represent actual facts, we have the interesting result that the star *a* is probably a member of an immense distant system, of which β Orionis is also a member, whilst the star *b* is an inconspicuous but comparatively near member of our galaxy." We have learnt from Dr. Gill that some observations which he made a few months later to test the truth of this result threw suspicion on the whole series of observations; and on careful examination an error was found in the reductions which entirely vitiates the result first announced. It is not surprising, therefore, that Dr. W. Schur, who repeated the observations with the Göttingen heliometer, found quite different results for parallax, viz. from star *a* $+0''\cdot135$, and from star *b* $-0''\cdot025$. (Ast. Nach. No. 3033.)

PARALLAX OF β AURIGÆ.—Prof. Pritchard has photographically determined the parallax of this star with reference to DM. +44°1322 (mag. 10·2) and DM. +44°1334 (mag. 8·8), and finds the results $+0''\cdot059$ and $+0''\cdot065$. "The identity of the parallaxes .... seems to indicate the probability that the parallax of β Aurigæ, here determined, approaches the absolute...... Adopting, then, Dr. Vogel's distance between the components of β Aurigæ, it follows that their greatest angular separation as seen from the Earth is $0''\cdot005$." The photographic method cannot be applied to ζ Ursæ Majoris because "the two components are separated by 14″, and practically when the exposure is continued long enough to obtain the impression of the faint stars of comparison, the two components coalesce in a manner to prevent accurate bisection, and no method that I have yet been able to devise to prevent this confusion of images is altogether satisfactory." We suppose therefore that the method of exposing the bright star for a few seconds only at the beginning or end of the long exposure devoted to the faint comparison stars (which, we believe, was tried at Oxford in the case of Polaris to avoid having too large an image) has been abandoned as unsatisfactory.

SPECTROSCOPIC BINARIES.—Prof. E. C. Pickering points out (Ast. Nach. No. 3034) a new development of the method for discovering double stars by their spectra. If the spectra of two close components, whose motion in the line of sight is not the same, are not similar, lines which occur in the one only will be displaced with reference to lines which occur in both. In particular if one spectrum be of the second type like our Sun, and the other of the first type, then the resulting spectrum will be recognized by the strong marking of the hydrogen lines common to both components; and on careful measurement these lines should be found displaced with reference to the other lines in the spectrum. Of course the strengthening of the hydrogen lines may be due to other causes than the presence of a faint component, and then there should be no displacement. The criterion for the faint component would be a displacement periodically varying. An examination of the spectra of bright stars by Miss A. C. Maury seems to show that ten stars have such a composite spectrum:—viz. γ Andromedæ, H.P. 650, ϵ Boötis, α Scorpii, and β Cygni, which are known doubles, and τ Persei, ζ Aurigæ, δ Sagittarii, 31 Cygni, and β Capricorni. The distant companions of the last two give separate spectra and are not here referred to. An example of the method is given in the case of α Canis Minoris, and the displacement is found to indicate a relative motion of 20 kilometres per second.

THE transit of Mercury on the morning of May 10 could scarcely have been satisfactorily observed in these longitudes under any circumstances, as the egress took place soon after sunrise. As a matter of fact the weather was very bad, and clinched the matter. At Greenwich it was raining slightly at sunrise. At Meudon M. Janssen reports bad weather. He suggests that at the next transit (1894 November 10) Dr. Huggins's method of photographing the corona should be tried when Mercury is near the Sun's limb.

THE Annual Visitation of the Royal Observatory, Greenwich, will take place on Saturday, June 6. Curiously enough the Moon has selected that afternoon for a partial eclipse of the Sun, commencing about 5 o'clock; and this will gladden those who believe that an eclipse can be better seen from an observatory than elsewhere.

COMET *b*, 1891 (*c*, 1884, WOLF).—Mr. Barnard found Wolf's Comet on May 3.

WE are glad to hear that the Danish Academy of Sciences and Letters has awarded a gold medal to Baron E. v. Haerdtl of Innspruck, for his memoir on a case of the problem of three bodies proposed by the Academy in 1889.

ERRATA IN No. 175:—On p. 169 line 8, *dele* the name of Mr. Knobel. On p. 170 line 24 *for* Mr. Downing *read* Mr. Maunder.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 177. JULY. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, June 12, 1891.

Lieut.-Gen. J. F. TENNANT, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. KNOBEL and A. M. W. DOWNING, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Downing. Seventy-eight presents have been received since the last Meeting. Amongst those calling for special mention are:—W. Harkness, 'The Solar Parallax and its related Constants,' presented by the U.S. Naval Observatory; Harvard College Observatory, 'Variable Stars of Long Period,' presented by the Observatory; O. A. L. Pihl, 'The Stellar Cluster χ Persei,' presented by the Author; 'Report of the Observations of the Total Eclipse of the Sun, Jan. 1, 1889, made by the Washington Observatory Eclipse Party,' presented by the Observatory; H. Raper, 'The Practice of Navigation and Nautical Astronomy,' 19th edition, presented by the Publishers; 'Siddhanta Siromani' (Hindu Astronomy), presented by Colonel Waterhouse; Pulkowa Observatory, 'Catalog von 5634 Sternen für 1875·0 aus den Beobachtungen am Meridian-Kreise,' von H. Romberg, presented by the Observatory; J. Kleiber, 'On the Determination of the Orbits of Meteor-Streams' [in Russian], with an abstract in English, presented by the Author.

A vote of thanks was accorded to the donors of the presents.

Mr. Knobel read a paper by *Mr. H. C. Russell*, entitled "Notes on some Star Photographs recently taken at the Sydney Observatory."

The previous photographs of the Milky Way round η Argûs sent to the Society by Mr. Russell were under-exposed, but with the same camera and plates he has since obtained a photograph with eight hours' exposure. This makes the aspect of the region of the Milky

Way very different from that shown on the previous photograph. The increase in the number of stars shown is very obvious; but it is satisfactory to note that little details—dark spots and lines visible in the fainter negative—are reproduced in this. The nebula itself is much brighter, the details are more defined, the outlying wreaths of nebula extending over a much wider space than has appeared in any drawing or previous photograph. Although several portions seem to show that a still longer exposure is required, the dark oval space near the southern extremity of the nebula shows no increase of nebulous light with increase of exposure. In addition to the photographs with the 6-inch lens in the portrait-camera, Mr. Russell has made a number of long exposures on η Argûs with the "Star-Camera" or charting telescope, and these show that the central portion of the nebula around η Argûs has a most complex structure, similar to that around θ Orionis. More extension is shown in the portrait-camera negatives than can be seen in negatives with the star-camera exposed for the same time. It was in the dark oval space that one of the brightest and most conspicuous parts of the nebula was situated when Herschel was at the Cape; and the fact that it has disappeared is a significant commentary on the course he adopted in not only drawing the nebula, but in marking out defined and conspicuous portions of the nebula by the conspicuous stars. In 1871 Mr. Russell noticed, with a $7\frac{1}{4}$-inch telescope, that the part of the nebula at the intersection of the lines $+50^s$ and $+3000$ had disappeared, and the $11\frac{1}{2}$-inch refractor also failed to show it. It is absent from the best of the photographs, which show fainter nebula and hosts of stars that Herschel could not see. For example, in the great oval in the southern part of the nebula Herschel saw four stars, while the photograph shows twenty, and where he saw one star the photograph shows ten. There is therefore the strongest evidence that between 1837 and 1871 a well-defined and brilliant portion of the nebula about η Argûs has disappeared. During March 1891 Mr. Russell has taken photographs to trace the Milky Way in the great rift in Argûs noticed by Herschel. Three photographs cover this region. One, with its centre at R.A. $7^h\ 50^m$ and Decl. 47° 47′, is covered with stars almost uniformly, although the eye sees no Milky Way in that part of the sky. Another plate shows stars uniformly over the rift between R.A. $8^h\ 10^m$ and $9^h\ 10^m$, while a third shows the same appearance from R.A. $10^h\ 10^m$ to R.A. $10^h\ 30^m$. The whole rift from $7^h\ 20^m$ to $10^h\ 30^m$, which to the eye is void of stars, is shown by the photographs to be uniformly covered with them. A photograph of the "Nubecula Major" was taken with $6^h\ 17^m$ exposure on Feb. 9, 1891, with the star-camera, and the negative shows the central part of the nebula overexposed, but the surroundings show out faintly as a most complex structure. These are very similar to the structure visible to the eye, that is, a series of loops arranged symmetrically with regard to the central mass. In other prints the great groups are very concentrated, without sign of spiral structure, and these clusters are roughly parallel to each other and separated by dark bands. In places the stars number 5000 or 6000 to the square degree, so that it is not surprising to find them run together into blotches of light when photographed with the portrait-camera.

Dr. Gill. I should say with reference to Mr. Russell's photograph of the η Argûs nebula, taken with an exposure of five hours, which I had the pleasure of examining a few days ago, that it seemed to me to be one of the most beautiful photographs of any nebula that we have ever seen. Not only is the nebula shown in exquisite detail, but the stars are so surprisingly well defined throughout the nebula. I know the η Argûs nebula is exceedingly difficult to photograph; it does not contain so much blue light as most others, and on that ground a long exposure is required to procure effect. Short exposures will not give any satisfactory photographs of this nebula.

There is one point in the paper which strikes me as specially interesting in connection with the subject to which I referred the other day at the Astronomical Association, viz. the difference between the visual and photographic brilliancy of stars in the Milky Way. From this paper we learn that the Milky Way is shown by photography to extend right across certain regions apparently bare to the naked eye. That is a point of great interest.

In a paper recently published in the 'Astr. Nach.' by Professor Pickering, he says that the brighter stars in the Milky Way are chiefly of the Sirian type, and that explains a fact which puzzled us very much in photographing the southern heavens. Professor Kapteyn found that the photographic estimations of magnitude differed from the eye estimations of magnitude in the rich parts of the sky, that is to say, that the estimates photographically of the magnitudes of stars in the rich parts of the sky were from $\frac{1}{2}$ to $\frac{3}{4}$ of a magnitude brighter, as compared with the eye estimations of magnitude, than they were in the barer regions of the sky. One of two explanations was possible. The first one would be that the eye estimates were low in the rich regions, because of the brighter background, which may affect all eye estimations. The difference seemed too great for this to be the case, especially when considerable power was employed on the telescope. The second explanation is Prof. Pickering's spectroscopical one; and this brings out the fact that in the brighter stars of the Milky Way there is a systematic difference in spectra, and the difference between the photographically determined and eye-estimated magnitude is always found with stars that have the Sirian type of spectrum. Thus the Cape Durchmusterung appears to show that the faint stars as well as the brighter stars of the Milky Way are chiefly stars of this type; and therefore probably the Milky Way is a structure in the heavens by itself, and probably, according to the probable order of evolution of stars, the Milky Way may be a more recent creation or, at least, a creation in an earlier state of development than other portions of the heavens. This is a very far-reaching question, which of course will require the most thorough and complete examination and development; but it is one that affords an object for research of the very highest importance and interest in Astronomy.

Mr. Ranyard. In the large photograph of the η Argûs nebula which Mr. Russell has sent us, taken with the camera with which the survey of the heavens is to be made, there are most remarkable dark areas and channels, one of which branches like a tree. In other parts of the Milky Way we have already seen some dark areas, but on a smaller smaller scale, and some dark channels bordered with stars. Such dark spaces were known and described by Sir William Herschel; but a most remarkable great dark branching channel bordered by stars is shown in this photograph. Of course the one that goes through the η Argûs nebula was already well known. I should like to remark, in connection with what Dr. Gill has said, that many parts of the Milky Way seem to have come out very different on the photographs from the way in which they are seen and drawn by observers. One of the most striking instances is the remarkable photograph which Mr. Russell has sent us of the "coal-sack" region, which we know is strikingly visible to the naked eye. At first sight of the photograph you wonder where the coal-sack is, but when you look more closely you can trace the outline which has been drawn so often. We know that there is a cluster of red and blue stars in this region, and that the whole of the Milky Way is a region of red stars. The photographs certainly seem to render it probable that it is also a region of blue stars, or stars the general light of which is more actinic than the light of stars over the rest of the heavens.

The President. We must return our very grateful thanks to Mr. Russell for the very valuable photographs he has sent us, which many of you will have the opportunity of examining at your leisure. They are extremely beautiful photographs and they speak very highly, not only of his work, but of the work of Sir Howard Grubb, who made the lenses, for the star images are most beautifully defined.

Dr. Dreyer read a paper "On the Proper Motion of twenty Southern Stars." He had examined the more remarkable cases of discrepancies between the results of earlier and recent meridian observations of stars in the region of Schönfeld's southern 'Durchmusterung,' which might arise from proper motions, but only twenty cases could be determined with absolute certainty. It was interesting to note that there was not a single star catalogue published now-a-days without a number of proper motions of faint stars being brought to light. By continuing these investigations and getting together as many cases as they could find of proper motions of faint stars they would in the course of a few generations obtain very valuable material for the determination of the true system of the stars, whatever it might turn out to be.

The President. There is no doubt this question of the proper motion of stars is a most important one, and one which we shall only get the true solution of when we get the new photographic charts; but so long as you have to reduce thousands of stars with many errors amongst themselves in order to get a few proper motions, you can hardly say you have in any way nearly exhausted the question.

We have to-night amongst us a very eminent French physicist, our Associate, M. Cornu. I understand that he has something interesting to say to us. I am quite sure I am expressing the feelings of everybody in this Society when I welcome M. Cornu, and I will ask him to let us hear something of what he has been doing across the Channel.

M. Cornu (who spoke in French) was invited by the President to describe the apparatus by means of which he is measuring the mean density of the Earth.

· This work was undertaken in collaboration with Mr. T. B. Bailli in 1867. A torsion balance similar to that of Cavendish, and those afterwards employed by Baily and Reich, immediately gave satisfactory results if an accuracy of 1 °/₀ is considered sufficient. But a much greater precision was desired, and it became necessary to eliminate a large number of causes of error both *accidental* and *systematic*; which explains why these experiments have taken so much time. It must be remembered that the total force to be measured is extremely small (about a *thousandth of a milligramme* in our apparatus), and thus many disturbing influences ordinarily negligible are here of the same order as the force itself; and if we desire an accuracy of anything like 0·1 °/₀, we must consider all disturbing forces of the order *one millionth of a milligramme.* The following are the chief improvements of the Cavendish apparatus which we have introduced:—

1. *Shortening of the arm of the torsion balance.*—It is demonstrable that, other things being equal (attracting and attracted masses, distances between them, period of oscillation of the arm), deviation due to gravity is inversely proportional to the length of the arm. This arm has been reduced to $0^{\mathrm{m}}\cdot 50$; the suspended balls weigh 100 grammes, and the period of oscillation is about 7 minutes.

2. *Reduction of the attracting masses* to 10 kilogrammes, so that the manipulation is now quite simple; and to avoid errors in placing them, the method has been adopted of using globes filled with mercury which can be pumped into one or other pair of globes whose position is not otherwise disturbed.

3. *Registration of the whole oscillation of the balance arm* and investigation of the law. The transits of the millimetre-scale divisions, seen reflected at a distance of 5 metres, are registered on a chronograph in the usual way. The oscillations of the arm may thus be compared with the law

$$x = \mathrm{A}e^{-at} \sin 2\pi \left(\frac{t-t_0}{\mathrm{T}}\right),$$

which results from integrating the differential equation of motion of a solid moving under the action of two sets of forces, (*a*) proportional to the displacement (torsion, variation of distance of attracting masses, &c.), which are the subject of investigation, (*b*) proportional to the velocity (resistance of air, torsion friction, &c.), which are of the nature of disturbing forces. The method of complete registration has led, however, to the detection of a third group of forces depending on the square of the displacement. An inspection of the chronographic record showed the existence of these forces very clearly, and gave means for measuring them.

4. *Use of a torsion thread of annealed glass.*—This precaution has got rid of those anomalies which always appear in torsion balances if a twisted thread be employed, or one which has been wound on a bobbin (instability of the zero, progressive and rapid alteration of zero, &c.). It is indispensable also to use a very long thread; for we thus minimize or even wholly eliminate errors due to the attachments of the ends of the thread and elastic retardation.

5. *Elimination of electric disturbances* due to different causes (particularly those due to the variation of potential of the surrounding air, above and below the apparatus). All parts of the apparatus are put in good metallic connection—the suspended balls, the arm, thread, and metallic case enveloping the arm.

6. *Elimination of thermal disturbances* due to differences of temperature in the neighbourhood. These produce currents of air, which betray themselves by hydrodynamic effects playing the part of regular forces. Thus a candle placed at a distance of some centimetres from the arm, protected by a thin zinc box, *appears to attract* the neighbouring ball. This effect is exactly analogous to that observed in the radiometer by Mr. Crookes, with the important difference that the apparent action of a warm body is an attraction instead of being a repulsion as in the radiometer. It appears whenever the case protecting the arm is a bad conductor of heat; and disappeared when the case of thin zinc was replaced by one of thick red copper, so that the conduction of heat due to accidental sources is very rapid, and complete uniformity of temperature is secured. The zero of the torsion balance is now astonishingly constant.

Little now remains to be done to complete these long and delicate investigations, and the authors hope soon to be able to give their definitive result and a satisfactory estimation of the probable error.

The President. I do not know if any Fellow of the Society would wish to ask any question or comment upon what M. Cornu has said. I think it is very very rarely indeed that we have had in this room, or in any room in London, a description of a very difficult physical investigation given to us in terms which are so strikingly expressive and so easily understood even by those who are not familiar with the sort of work that is spoken of. I think we owe M. Cornu our very cordial thanks for the trouble he has taken, and we only hope that his reception here to-night will encourage him to visit us again.

Dr. Gill. I do not wish to appear obtrusive, but perhaps I may be allowed to add one word to what M. Cornu has said, because I believe I am the only person in this room who has actually seen the force of gravity demonstrated by M. Cornu himself. I had the pleasure of seeing his apparatus and of looking through the collimator of it, and I can say that the scale was absolutely at rest, it had no motion whatever, there was even no tremulous motion. But immediately the mercury was run from one pair of bulbs into the opposite pair the force of gravity was visibly demonstrated; the image at once began to move, and the movement of the scale-divisions across the collimator-wire was as exact and smooth as the movement of a star in the field of a transit-instrument and as easily, or even more easily, observable.

Mr. Stone. I hope M. Cornu will have the kindness to draw up for insertion in the 'Monthly Notices' an account of his experiments which he has so clearly explained to us this evening. Such an account would be of permanent value and interest, not only to those who have had the good fortune to he present, but to the other Fellows of the Society.

Prof. A. S. Herschel. There is a point of considerable physical interest noticed by M. Cornu in this description of his work, which, from his distinguished skill as a physicist as well as great eminence as an astronomical investigator, corroborates remarkably what has also been observed very recently by English experimenters. About a year ago Sir William Thomson, wishing to examine the state of electrification of the air in certain rooms of the new College of the Glasgow University with a quadrant electrometer, found it so extremely variable that the cause of its rapid changes could not at first be discovered. It was, however, soon suspected, and then proved by very numerous experiments, that the effect was produced by lamp-flames burning in the rooms, and even very rapidly by the flame of the single small lamp used to throw a beam of light upon the electrometer-scale.

The electrifying effects of a variety of burning materials, such as oils, gas, and charcoal, upon the air were made a subject of research with results communicated last year to the Glasgow Philosophical Society, showing the effects in nearly every case, particularly in that of burning charcoal, to be very considerable. It is very interesting to learn that disturbances traced to scattered electrification were met with in M. Cornu's work. The experiments referred to might lead us to suppose as probable that even the small flame used at first by M. Cornu in the observing-room to throw a beam of reflected light upon the reading-scale might produce, perhaps, a considerable part of the electrical difficulties which he was so well able to overcome.

The Astronomer Royal read a paper entitled "Preliminary note on the change of Personal Equation with Stellar Magnitudes in

Transits, observed with the Transit-Circle at the Royal Observatory, Greenwich." This question is one of some importance to practical astronomers. It has been brought into considerable prominence lately in Dr. Auwers's discussion of the observations of Victoria and Sappho for the determination of solar parallax. The reduction of stellar magnitude by most methods ordinarily employed was mixed up with the question of the possibility of diffraction affecting the observations, and he had at first hoped that they would have been able to get observations at Greenwich with the personal equation machine that would be free from these sources of error. Other work had, however, interfered with this, and as it would have been necessary to adapt photometric appliances to the personal equation machine, and this would have involved considerable delay, it was decided to determine the change of personal equation with stellar magnitude by means of wire-gauze screens placed in front of the object-glass of the transit-circle. The effect of these screens in reducing apparent magnitude was estimated carefully by the observers and their estimations checked by the employment of diaphragms of known size. Transits of stars were observed part with screened object-glass and part with unscreened, and the deduced times over centre wire were compared. The general result was that with all the observers there was a tendency to observe stars with object-glass screened *later*, and that this was more marked in the case of the fainter stars; but there were some discrepancies which made one hesitate as to the exact interpretation of the results; for instance, when the object-glass was doubly obscured by the use of two screens, the change of personality was not found to be doubled, but actually less than for one screen. The paucity of observations with two screens might account for this anomaly, but it was not unlikely that the peculiar character of the diffraction image influenced the observers more than the change of magnitude. This character had recently been investigated photographically in connection with the Resolutions of the Permanent Committee for the Photographic Chart; and he would show on the screen the photographed image of a star when the object-glass was obscured by a screen. [The central disk was surrounded by four well-marked radial bars, which, however, did not visibly extend to within some minutes of arc of the central image.]

There followed some desultory conversation on the use of the gauze screens for the Photographic Chart.

Mr. Turner. I should like to state what my difficulty is, and perhaps Dr. Gill will be able to clear it up. At the Paris Conference the question of these screens was mixed up with the one as to whether time of exposure was equivalent to intensity of the stellar image on the photographic plate, the latter point having been most distinctly expounded by Captain Abney. It seemed to me that with this as a basis we had a more reliable method of photometry than we had ever had before; yet these screens were, so far as I could gather, introduced for the better determination of magnitudes. That implied that there must be some advantage either in principle or convenience in the use of the screens. But even if the law of exposure is not absolutely true, the divergence from the law might have been defined quite clearly, and so the intervention of the screens rendered unnecessary. Again, photography gives different photometric results from eye-observations, and yet it is proposed to investigate the properties of the screens by eye photometry!

Dr. Gill. I think the discussion which has taken place to-night shows pretty clearly, if it shows anything at all, that photometry is a very difficult question. If you do not use the screen you put 18 unfortunate astronomers into a difficult position. You have 18 astronomers setting to work to determine a photometric problem, possibly all doing it in different ways, and possibly obtaining different results. I am not entering into the question as to how the photometric absorption of a screen is to be determined, I am quite sure that our friend M. Cornu would readily undertake it; but if you have once determined the photometric absorption of a single sample screen, and can produce 18 of those screens exactly alike (as you can from a web of wire cloth), then you have saved all these astronomers the trouble of separately determining magnitudes. The great beauty of the screen is that once you have determined the necessary screen to produce an absorption of two magnitudes, you have only to make 18 screens exactly the same, and the work is done for all astronomers.

Mr. Turner. Will they vary with different object-glasses?

Dr. Gill. I believe that is a physical question and capable of a physical solution. If the object-glasses are made alike, with minimum focus for G, and well corrected for spherical aberration, it is quite certain that the absorption will be alike for all.

The Astronomer Royal. I would like to clear up the points in which Dr. Gill differs from Mr. Turner and myself. On the supposition that 18 astronomers have identical object-glasses and identical atmospheric conditions, they then have to determine in what time they can get the 9th mag. stars with the screen, and use this as the exposure for 11th magnitude with unscreened object-glasses. An alternative method would be to give them certain typical stars whose magnitudes are determined visually by ordinary photometric methods, and to let them experimentally determine the exposure necessary to photograph these stars.

Dr. Gill. That would be a most excellent plan.

The Astronomer Royal. The objection to it is that you have to determine a larger number of stars than is necessary with the screen. The whole question is, would it not be better to determine the typical magnitude stars first, and then let unfortunate observers find what exposure is necessary to photograph those stars than to determine the difference by the use of screens? I agree with Mr. Turner that the introduction of the screens does not help us in the matter, and never will help us. Nobody can be certain that the results obtained by the screens will be the same for all object-glasses. I must also take exception to Dr. Gill's statement that

18 or 19 exactly similar screens could be easily made. I have seen several of these screens, and there are great discrepancies between them. I am very much afraid of these screens.

Mr. Plummer. The point the Astronomer Royal has raised was so strongly felt at Oxford that Prof. Pritchard has proposed to determine definitely the magnitude of certain stars of approximately the 11th mag., independently and without reference to any grating in front of the object-glass. There may be some difficulty in determining with accuracy magnitudes of stars as faint as the 11th, but these difficulties can be overcome.

The President. We have wandered greatly from the subject which was originally started, from the change of personal equation to the magnitudes of stars. My first acquaintance with these diffraction phenomena was from Sir John Herschel's paper in the 'Encyclopædia Britannica,' written something like sixty years ago, and it so happens that early in this half-century—1853 or thereabouts—I tried, when surveying, to dull the light of heliostats by a reticulated screen. The screens which I used were made of the nicest woven cloth I could get, and I was under the impression that by using these I could conveniently observe in succession the very bright lights at stations where the Sun was clear and the fainter ones at those where the Sun was clouded over. I soon had to give it up, for I found it was impossible to take an angle when A was bright and B was dull to agree with those where A was dull and B bright. It was painful to observe the objects varying in brilliancy, but I got consistent results. With the screens I did not get consistent results when I came to dull the bright objects in this way, and that was clearly due to some introduction of personal error. I am pretty sure that in observing the distance between a bright star and a faint one if a bright star is dulled down by a screen, there is some effect, and I want to ask Dr. Gill how he does with the heliometer when he dulls one star down in that way.

Dr. Gill. With regard to the effect of the screen, I may say we have made thousands and thousands of observations. I have reduced Sirius down to the 8th magnitude, the result being that you cannot by any possibility distinguish the reduced image from that of an unreduced 8th mag. star, the images are so good that probable error of the complete observation of distances on one night is only about $\frac{4}{100}$ths of a second of arc. When you reduce a bright star down to a faint one you get diffraction-rays; but if you use a fine screen these diffraction-rays are so minute that with a high power and the diffraction effects so far removed from the central image I defy the most keen-sighted astronomer to distinguish between the image of a star so reduced and the image of a companion star of the 8th magnitude.

The President. The images we have are not like those you see. Those we have seen are less than 7 minutes from the centre.

Dr. Gill. No, in the photograph of the image of α Lyræ, which was taken at Greenwich, and which has been projected on the screen, only a single fold of wire gauze has been employed, reducing the image perhaps 2 magnitudes only. The diffraction-effects are therefore very bright, in the case of a bright star like α Lyræ; but you will observe they occur at about 7 minutes of arc from the central image, and they would be entirely invisible in the field of view of an eyepiece of high power, even if they were considerably closer than 7 minutes from the central image, for the field of such an eyepiece is much less than 14 minutes of arc in diameter—probably only 3′ or 4′.

The following papers were announced:—

H. C. Russell. "Notes on some Star-photographs recently taken at the Sydney Observatory."

S. W. Burnham. "The Companions to Regulus."

Rev. S. J. Johnson. "Probably Early Observation of an Immersion of Titan."

J. L. E. Dreyer. "Proper Motions of twenty Southern Stars."

K. D. Naegamvala. "Observations of the Transit of Mercury, 1891, May 9."

A. V. Nursingrow. "The Transit of Mercury, 1891, May 9, as seen at Vizagapatam."

F. McClean. "Photographs of Metallic Spectra."

E. J. Stone. "Observations of the Moon made at the Radcliffe Observatory, Oxford, during the year 1890, and a Comparison of the Results of the Tabular Places from Hansen's Lunar Tables."

Radcliffe Observatory, Oxford. "Observations of the Partial Solar Eclipse of 1891, June 6."

A. Marth. "Data for computing the Positions of the Satellites of Jupiter 1891. With tables of the Inequalities."

A. Marth. "Ephemeris for Physical Observations of the Moon, 1891."

Dr. R. Copeland. "Observations of the Transit of Mercury, 1891, May 9, made at the Royal Observatory, Edinburgh."

T. Lewis. "The Orbit of 36 Andromedæ (Σ 73).

W. H. M. Christie. "Preliminary Note on the Change of Personal Equation with Stellar Magnitude in Transits observed with the Transit-Circle at the Royal Observatory, Greenwich."

Richard Edward Synge Cooper, Hillmorton Paddox, Rugby; *Hugh Frank Newall,* Trinity College, Cambridge; *Samuel H. Ridge, B.A., F.R.G.S.,* 275 Victoria Parade East, Melbourne, Victoria, were duly elected Fellows of the Society.

The following Candidate was proposed for election to the Society:—

Lord Edward Spencer Churchill (proposed by the Earl of Rosse).

ROYAL METEOROLOGICAL SOCIETY.

THE concluding Meeting of this Society for the present session was held on Wednesday, June 17, Mr. BALDWIN LATHAM, M.Inst.C.E., *President,* in the Chair.

Mr. J. J. Colman, M.P., Mr. E. B. Duhoff-Gordon, B.A., Mr. G. E. Leon, Mr. T. de C. Meade, Assoc.M.Inst.C.E., and Mr. F. Russell, F.R.G.S., were elected Fellows of the Society.

Mr. A. J. Hands gave an account of a curious case of damage by lightning to a church at Needwood, Staffordshire, on April 5, 1891. The church was provided with a lightning-conductor, but Mr. Hands thinks that when the lightning struck the conductor a spark passed from it to some metal which was close to it, and so caused damage to the building.

Mr. W. Ellis read a paper "On the Mean Temperature of the Air at the Royal Observatory, Greenwich, as deduced from the Photographic records for the Forty Years from 1849 to 1888." The paper includes a table of monthly means as deduced from 24-hourly readings from the photographic register on each day, and contains also an account of the way in which, at different times, Greenwich mean temperatures have been formed. It is further pointed out that neither ten nor twenty years is a sufficiently long period, as some persons have supposed, for the formation of temperature-averages in this country, and comparisons are given showing that the temperatures determined at Greenwich during the period covered by the table truly represent the general variations of temperature and are not affected by the growth of London in modern times, as has been by others imagined.

Mr. Ellis also read a paper "On the Comparison of Thermometrical Observations made in a Stevenson Screen, with corresponding Observations made on the Revolving Stand at the Royal Observatory, Greenwich." From this it appears that the maximum temperature in the Stevenson screen is lower than that of the revolving stand, especially in summer, and the minimum temperature higher, whilst the readings of the dry and wet-bulb thermometers on both the screen and the stand, as taken at stated hours, agree very closely together. The results of special experiments made on days of high temperature and bright sunshine, included in this paper, show that the readings of the thermometers exposed on the revolving stand (known as the Glaisher stand) are not influenced by radiation from the ground and the surrounding buildings in the way that has been frequently affirmed.

Mr. W. F. Stanley exhibited and described his "Phonometer," which is really a new form of chronograph designed for the purpose of ascertaining the distance of a gun, from observations of the flash and report of its discharge, by the difference of time that light and sound take in reaching the observer. The instrument can also be used for measuring the distance of lightning by timing the interval between the flash and the report of the thunder.

A paper was also read by Mr. A. B. MacDowall on "Some Suggestions bearing on Weather Prediction."

Selenographical Notes.

Delambre.—Though not far removed from the lunar equator and central meridian, and therefore conveniently placed for observation, this fine ring-plain has not attracted much attention from observers, despite the fact that it is certainly one of the most interesting objects of its class, and is, moreover, situated in a district which includes features of a very noteworthy kind.

The shadow of the lofty western border, which towers nearly 15,000 feet above the floor, renders it impossible to see much of the interior till the Sun has attained a considerable altitude, or till the morning terminator has moved as far towards the east as West long. 5°, or farther. At this phase the centre of the formation is just in sunlight and the bright inner slope of the eastern wall is seen to the best advantage. In this, as in so many other of the so-called ring-mountains, the more or less irregular hexagonal outline of the loftiest section of the border is very noticeable. It is not so evident under low powers, but under a magnification of from 200 to 400 it is so obvious that it can hardly escape the notice of any but the most superficial observer. As this peculiarity of structure does not harmonize very well with existing theories as to the formation of lunar rings, it awaits the attention of the framers of hypotheses in the future.

It would be difficult to find among objects of a similar type and size a more remarkably complex wall than that of Delambre. The inner slope consists of a succession of terraces or concentric rings, which, however, are to a great extent discontinuous. Below the culminating ridge on the south-east there is a deep hollow or valley whose lowest recesses remain sunless long after all other depressions on the slope are illuminated. Schmidt (in whose chart it alone appears) represents it as a "pocket," or row of inosculating craters, and as a prominent feature. One or two of the secondary ridges or terraces on the south-west border are very lofty,—standing out amid the shadow of the western wall at an early stage of the lunar day. Neison shows nothing of these terraces, and Mädler draws only one of them on the western side and none on the other. The floor includes no very lofty heights. Schmidt has a large central elevation with two craters on the eastern and a peak on the western flank of it. Mädler and Neison draw two parallel rows of hills trending from north to south. On the night of April 15 this year, 8^h to 9^h 15^m, a power of 350 on my 8½-inch Calver-reflector showed a well-marked ridge running from the large bright crater on the north border to two deep circular pits, filled with shadow, near the centre, answering to Schmidt's craters. These objects were connected by a rill-like valley which possibly represents the depression between the hills shown by Mädler. It is strange that neither he nor Neison show the pits, though they are very prominent. No other details were seen in the interior.

The two fine deep crater-plains, Theon senr. and Theon junr., the names of which are, by the way, transposed in Schmidt's Chart, were at this time filled with shadow, and all the surrounding features displayed with great distinctness under oblique light. About midway between them lay what appeared to be a mountain-group much involved in shade and exhibiting detail difficult to define satisfactorily. On the following night the appearance of this object was entirely transformed, two shallow rings occupying the site, showing how important it often is not to decide as to the actual character of a formation until it has been seen under more than one phase of illumination. Schmidt draws curved ridges, inclosing craters, here, but the enclosures noted were far larger than his. Neison shows nothing, and Mädler only two hills on the site.

Running from the north side of Taylor *a* to the east of the Theon crater-plains and beyond, towards the north, is a prominent mountain-range which attains its greatest elevation at a peak marked *a* on Neison's Map 21, a little south-east of Theon senr. At the eastern foot of this range, close under the peak, and at the time of observation partially involved in its shadow, I noted two brilliant white circular spots in close contiguity. On careful examination the more easterly of these objects turned out to be a crater filled with black shadow and casting a shadow. I could not be so certain as to the true character of the companion, that nearer the foot of the range, though at times it seemed to have a dusky (not black) centre. These twin white spots align with Theon junr. They cannot be traced in any of the maps, though Schmidt and Neison show a small crater near the site which may possibly represent one of them. They were so very conspicuous, both being at least 6° on the adopted scale, that one cannot understand why both are not inserted and specially referred to.

There is a marked tendency in the hills and ranges on the south and north of Delambre to a north-east to south-west direction. This direction is not rectilineal but slightly curved, the convexity being towards the south-east. Three very prominent parallel ranges abutting on the south-west wall of the formation display the same tendency. Neison speaks of the ruins of a small ring-plain, Delambre *a* (with a central peak), on the north side of this formation. It can only be well seen at an earlier stage of illumination than that which obtained when the above observations were made. It lies between Theon senr. and the wall. I have not noted the central peak.

Beaumont House, Shakespeare Road,
Bedford, 1891, June 19.

THOS. GWYN ELGER.

Stellar Parallax, as determined by a Transit Instrument[*].

We have to congratulate Prof. Kapteyn on the successful inauguration of a new method of determining relative stellar parallax, by the employment of a transit instrument for the observation of differences of Right Ascension between the selected star and neighbouring stars of comparison. Nearly thirty years ago Prof. Auwers sought to determine the parallax of Groombridge 34 by observing the difference of Right Ascension from contiguous stars, employing the equatorial of the Gotha Observatory. The conclusion at which that astronomer arrived was that the method adopted was distinctly inferior to that depending on heliometrical measurements; and the plan has been since abandoned. The measurement of the differences of declination by means of an equatorial has been applied by several astronomers to the investigation of stellar parallax with greater or less success, but Prof. Kapteyn's process appears to possess some advantages which that method cannot claim. The method of differences of declination usually necessitates measurements at very different altitudes, and is consequently accompanied by proportionate uncertainty in the calculation of the small corrections for differential refraction, eliminated, it is true, to some extent by a judicious selection of the stars of comparison. But unless the star has a considerable latitude and the parallactic ellipse approaches a circle, the factors of parallax are small and inadequate.

Prof. Kapteyn's process is quite distinct from that which has been recently applied by Dr. Belopolsky with great apparent success. The Pulkowa astronomer has discussed the Right Ascensions of certain circumpolar stars, observed above and below the Pole when, the observations being made at different seasons of the year, a large factor of parallax is generally available. But if any systematic errors exist in the catalogue of clock stars employed in the reduction of the observations, which is the more possible as the fundamental stars are separated by approximately twelve hours of Right Ascension, these systematic errors are likely to be transferred to the deduced parallaxes. The plan of Prof. Kapteyn is free from this source of error, for the principal feature of this method consists in observing with a transit instrument, connected with a chronograph, the transits over a number of wires of the star whose parallax is sought, and of two others as nearly as possible symmetrically situated, at slightly greater and less declination. The difference of declination should not be great, in order to minimize as far as possible any uncertainty in the determination of the errors of the transit instrument. The differences of Right Ascension should be inconsiderable, to avoid a possible alteration in the clock-rate. The stars should also be as nearly as possible of the same magnitude, so that a personal equation, traceable to difference of magnitude which has been frequently observed, may not have a detrimental effect. Prof. Kapteyn employed a gauze screen in front of the object-glass to reduce the more brilliant of the stars to uniformity of apparent magnitude, and no doubt much of the success attending this first attempt is due to the care with which he foresaw and eliminated sources of possible error.

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[*] Bestimmung von Parallaxen durch Registrir-Beobachtungen am Meridiankreise von Prof. Dr. J. C. Kapteyn.

The more nearly these conditions are fulfilled the more favourable are the conditions for the determination of parallax. Practically it has been found possible to so select the stars of comparison that the mean of their declination rarely differs 5′ from that of the principal star. The differences of Right Ascension have given rather more trouble; sometimes it has been found necessary to sacrifice some of the wires in the transit owing to the stars following too closely; but these are details into which we cannot enter too minutely.

Before describing the sources of error and the treatment of the observations, it may be as well to explain that the stars on which the experiment has been made are all remarkable for considerable proper motion, are confined to a zone of 21° in declination, and do not transit very far at their upper culmination from the zenith of Leyden, where the observations were made. Owing to duties connected with University work, the observations had to be made in the Easter and Christmas vacations, and under these circumstances it might be anticipated that the stars would not be displaced by the total amount of the parallax. Nevertheless, by choosing stars which crossed the meridian near the evening twilight at one time of the year and near the morning twilight at the other, Prof. Kapteyn has arranged that the observations are accompanied with a very significant factor. In considering the applicability of the general method, it is important to bear in mind that the factor of parallax will be larger, and the method consequently the more favourable, the nearer the star is to the solstitial colure, for here the circle of declination is parallel to the ecliptic and the total parallactic displacement is parallel to the equator.

Among the sources of error which may be anticipated there is, of course, an erroneous determination of the ordinary corrections of the transit instrument; but since the particular zone in which each star occurs extends over so small a declination, this may be dismissed, as in any well-appointed observatory such corrections would be always under control. Of more importance is a possible change in the clock-rate, and the determination of this rate, with the necessary accuracy, has given Prof. Kapteyn some anxiety. It had been intended in the preparation of the scheme to determine the clock-rate from stars of approximately the same declination, but separated as far as possible in Right Ascension. Bad weather prevented this arrangement being fully carried out, and it has been sometimes necessary to employ the daily rate, which, there is reason to believe, differs sensibly at different periods of the day.

Deriving the hourly rates of the clock in three different ways, Prof. Kapteyn finds that the difference between these rates in spring and winter is as follows:—

	Spring.	Winter.
I.	$-0^s.0117$	$+.0106$
II.	$-.0099$	$+.0078$
III.	$-.0124$	$+.0110$

This result implies that with the clock-rate determined in the ordinary manner there will be a discrepancy of $0^s.022$ in the length of an hour between the spring and winter observations, or, in other words, if the length of the hour be by the ordinary methods correctly determined in spring, the winter observations will contain an error of $0^s.022$. An inquiry of this character has evidently a wider interest than the investigation of stellar parallax. Unfortunately, Prof. Kapteyn is not able to give an adequate explanation of this fact. It cannot be ascribed to variations in the transit errors, nor to alterations in the length of the chronographic record of the second, and another plausible suggestion which is submitted seems also insufficient. It is possible that the pendulum-rod experiences changes of temperature more rapidly than does the mercury cistern. The direction in which such a cause would operate is in that which has been observed, but the amount would probably be much smaller. We venture to make another suggestion, the possibility of which has not been discussed by the author. It is possible that the "latency" of the electromagnetic styli which mark the chronographic tape is not constant. Unfortunately we are unable to refer to the method by which the strength of the battery current is kept constant, and if no precaution is then taken it seems possible that a variation of temperature in the course of the evening, which would alter both the resistance and the electromotive force, might alter the interval in the discharge of the styli. Cases are on record in which the time between the completion of the circuit and the release of the armature amounts to 0·04 second, subject to variation depending on the condition of the battery. This cause would probably be insufficient to account for the whole of the observed quantity, but recent investigations at Leyden have shown that the variation of the hourly rates is probably smaller than that found by Prof. Kapteyn.

Many important points in the paper must be passed over for want of space, but it is desirable to record what degree of accuracy may be expected in this class of observation. In the first place we have the error of transit over the individual wires as determined from the reduction of each to the middle wire, and the error of a complete observation as derived from a preliminary solution of the equations of condition, giving to the observations equal weight. The results, divided into the four periods at which the observations were made, are as follows:—

	Error of a single wire.	No. of stars.	Error of an observation.	Mean No. of wires.	No. of observations.
1st Period...	0^{s}·0420	242	0^{s}·0168	19·2	391
2nd „ ...	·0438	205	·0178	19·8	263
3rd „ ...	·0395	192	·0173	19·6	257
4th „ ...	·0361	184	·0154	21·3	321
Mean......	·0404		·0168	20·0	

From the mean value 0^{s}·0404 of the error of transit over a single wire, the error in the observed difference of Right Ascension of two stars should be, when the transit is made over 20 wires,

$$\frac{0^{s}\cdot0404}{\sqrt{10}} = 0^{s}\cdot0128,$$

a quantity sensibly smaller than the 0^{s}·0168 given above. In this particular case the discrepancy may be explained by the fact that the errors for a complete observation are derived from a preliminary and not definitive discussion of the observations; but as a rule, Prof. Kapteyn, as it appears to us, distresses himself by the want of uniformity in the amount of the probable error as derived by different processes, and possibly by too great an insistance on the accuracy of the final results being gauged by the deduced probable error. This is, however, the less to be regretted, since it has induced the author to make an exhaustive inquiry into the freedom of the observations from systematic errors, and we cordially agree with his conclusion that systematic errors are not present in a noticeable degree.

Finally, a comparison is instituted between the accuracy of transit observations as compared with those made with the heliometer and with photographic processes. We can only give the results, and we believe Dr. Gill demurs to the value given in the case of the heliometer, and intimates that with his new instrument the errors are reduced by one half. The probable error in the determination of the place of the principal star, referred to the middle point of the two stars of comparison, is, when observed over 20 wires by the chronographic method, ±0″·132. The analogous quantity with the heliometer is +0″·075 and with the photographic process ±0″·086. The advantage in point of time required in making the observation is believed to be on the side of the chronograph.

Prof. Kapteyn has given a scheme by which the comparatively rapid determination of many parallaxes may be obtained. We beg to express a very sincere hope that the process may have a fair trial by some astronomer who has greater leisure than has Prof. Kapteyn, or, better still, we trust that that astronomer may soon find himself provided with suitable instruments to enable him to carry out an inquiry to which he has already applied himself to such good purpose and under somewhat adverse circumstances.

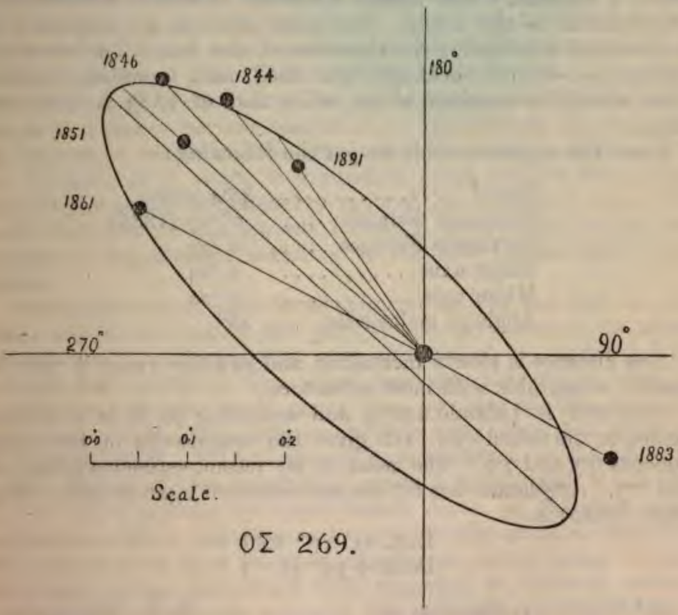
We give in the following table his final numerical results for the parallax of the stars he has investigated :—

B. B. vii. 81......	$\pi=+0''\cdot074\pm0''\cdot027$	B. B. vii. 104...	$\pi=+0''\cdot428\pm0''\cdot030$
θ Ursæ Maj. ...	$=+\ \cdot052\pm\ \cdot026$	„ „ 105...	$=+\ \cdot168\pm\ \cdot027$
B. B. vii. 85......	$=+\ \cdot064\pm\ \cdot022$	„ „ 110...	$=+\ \cdot030\pm\ \cdot027$
20 Leo. Min. ...	$=+\ \cdot062\pm\ \cdot029$	„ „ 111...	$=+\ \cdot016\pm\ \cdot032$
B. B. vii. 89......	$=+\ \cdot176\pm\ \cdot024$	„ „ 112...	$=+\ \cdot139\pm\ \cdot026$
B. B. vii. 94......	$=+\ \cdot101\pm\ \cdot026$	„ „ 114...	$=+\ \cdot038\pm\ \cdot042$
B. B. vii. 95......	$=+\ \cdot038\pm\ \cdot027$	„ „ 119...	$=+\ \cdot056\pm\ \cdot034$
Lal. 20670	$=-\ \cdot011\pm\ \cdot029$		

William E. Plummer.

The Orbit of OΣ 269.

There are very few measures of this close pair since it was first observed in 1844; and all the positions we have prior to 1883 are only single measures, scattered over a period of forty years, and some of them evidently with large errors. Engelmann, at the date last named, made a good series of measures considering the small aperture of his instrument, and these observations furnish the key to an approximate determination of the period. Without them



it would be impossible to say with certainty, taking the measures together, whether there was any appreciable motion at all.

I have lately measured this with the 36-inch telescope, and obtained a very satisfactory place for the companion at this time, although the distance is less than a quarter of a second. This pair is always difficult with moderate apertures, as the distance never exceeds about 0″·4, and most of the time it is much less. When the observations are brought together, it is apparent that most of them are of no use in a determination of the period. The errors are obvious when the whole series is considered. The following are all the measures which can be turned to any account:—

1844·31	218°·0	0″·33	OΣ	1 *n*
1846·39	223 ·8	0 ·39	OΣ	1 *n*
1851·39	228 ·9	0 ·33	OΣ	1 *n*
1861·26	242 ·8	0 ·33	OΣ	1 *n*
1883·41	61 ·4	0 ·22	E*n*	4 *n*
1891·26	213 ·4	0 ·22	β	3 *n*

I have carefully laid down these positions to scale, and after a number of trials have drawn the ellipse shown in the accompanying diagram, which makes the areas described substantially proportional to the times. The observations as a whole are well represented, considering the closeness of the pair. The observed distance in 1851 is evidently too small, and, according to this orbit, should be increased 0″·08, while that of 1883 is 0″·06 too large.

From this apparent orbit we get the following:—

Period	48·4 years.
Minimum distance	0″·07 (1886)
Maximum distance	0 ·42
Major axis	0 ·64
Minor axis	0 ·20
Angle of major axis ..	48°

The distance is slowly increasing, and in a few years it will be readily measurable with most refractors.

This star is Lalande 25074, and according to Heis it is just visible to the naked eye. OΣ gives the magnitudes of the components 6·5 and 7·0. The mean of my recent estimates gives 7·2 and 7·7. Argelander has 6·7 for the combined light of both. The place (1880) is:

$$\left.\begin{array}{l}\text{R.A. } 13^{h}\ 27^{m}\ 26^{s}\\ \text{Decl. } +35^{\circ}\ 32'\end{array}\right\}$$

Lick Observatory, 1891, June 10. S. W. Burnham.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The late Prof. Schönfeld on the New Star of 1006.

Gentlemen,—

Allusion was made in the June number of the 'Observatory' (p. 243) to the last paper written by Prof. Schönfeld, which was not published until after his lamented death, when it appeared in No. 3034 of the 'Astronomische Nachrichten.' May I offer you the following short account of its contents, which will perhaps interest many of the readers of the 'Observatory'?

Humboldt mentions, in the third volume of 'Cosmos,' a new star which has generally been supposed to have appeared in the year A.D. 1012 in the constellation Aries. This was from the statement of Hepidannus, a monk of St. Gall (who died in 1088, and whose annals extend from A.D. 709 to 1044); but Humboldt refers to another manuscript of the work of that writer which throws the dates six years further back, according to which the new star would have appeared in A.D. 1006. Now Prof. Schönfeld shows, from an Arabic and a Syriac chronicle, that the star was noticed in western Asia in a year stated to be 396 of the Hegira. The flight to Medina took place in 622; but as the Mohammedan Calendar is lunar and makes the year consist of 12 lunations or 354 days, the difference in reckoning the eras would diminish by twelve years in an interval of nearly four hundred years, and the year 396 of the Hegira would correspond to 1012 of the Christian era. The date therefore in the eastern chronicle agrees with that in the annals of Hepidannus, according to the best manuscripts of the latter.

But Schönfeld corrects Humboldt, not only in the date of the star's appearance but in the part of the sky where it was seen. The account in Hepidannus only states that it appeared "in intimis finibus Austri, ultra omnia signa quæ videntur in cœlo," a position which Aries could never have occupied at the place in question. Schönfeld points out that the word translated Aries in the Syriac record (this had already been indicated by Kirsch, one of the editors of the translation) means "sheep," and is not the word which denotes the constellation Aries, so that that rendering was only a conjecture. A very probable emendation of the original would make it the word used for the constellation Scorpio (in which several new stars have appeared at different times), which would correspond well with the expression of Hepidannus, and is therefore in all probability correct.

Yours faithfully,

Blackheath, 1891, June 10. W. T. Lynn.

The Eclipse of Columbus.

Gentlemen,—

At p. 58 of the Rev. S. J. Johnson's interesting little book, 'Eclipses Past and Future,' he refers to Riccioli as taking the lunar eclipse of 1493, April 2, as the one of which use was made by Columbus in frightening the natives into bringing him provisions, of which they had become neglectful. Riccioli does indeed give 1493 as the year of the eclipse of which "profuit non parum Christophoro Columbo notitia;" but assigns the day as October 22, when there could have been no eclipse at all, as the Moon was not quite full. In April 1493 Columbus was in Spain, after his return from the memorable voyage in which he discovered the West Indies; in October he was on his second voyage thither, and left the Canaries about the middle of that month.

Mr. Johnson goes on to say that it is doubtful on which of the voyages of Columbus the event occurred, adding: "The eclipse on the evening of March 1, 1504, has also been given as the correct date." Reference to the account by Ferdinand Columbus of his father's life will show at once that the incident took place on his last voyage, and that the eclipse must have been that of Feb. 29, 1504 (dated March 1 in 'L'Art de vérifier les Dates,' because the middle of the eclipse fell at Paris early on the morning of that day). In a note in the English translation (by Smyth and Grant) of Arago's 'Astronomie Populaire,' this date is correctly given; but Columbus is erroneously supposed to have been at the time in Hispaniola, whereas he was really on shore at Jamaica, having no means of leaving it on account of the shattered state of his ships. Many of his men too had mutinied and deserted him, and he was anxiously expecting that relief which at last tardily arrived, from the Governor of Hispaniola. Yours faithfully,

Blackheath, 1891, April 10. W. T. Lynn.

Epitaph of Maskelyne.

Gentlemen,—

In Rigaud's exhaustive account of the life and correspondence of Bradley, the epitaph of that distinguished astronomer (whose merit was so early discerned by his illustrious predecessor at Greenwich) in Minchinhampton Church is fittingly recorded. But I do not think (at any rate I cannot find) that that of Maskelyne has been published anywhere. He was Astronomer Royal during a longer period than anyone else who has held that office, slightly longer than Sir George Airy, who had not quite completed forty-six years at the time of his resignation in 1881, whereas Maskelyne was appointed in the month of February 1765 and he remained in office until his death in February 1811. The biographical notices of him which have hitherto appeared are exceedingly meagre. It is well known that he was of Purton, near Swindon, in Wiltshire, and was buried there. Thinking it might be of interest to the readers of the 'Observatory,' I wrote a few days ago to the present Vicar of Purton (the Rev. J. Veysey), and asked him to favour me with a copy of the Maskelyne epitaph there. He kindly and promptly complied with my request, and informs me there are two inscriptions, one in English on the tombstone in the churchyard, the other in Latin on the monument in the Maskelyne Chapel in the church itself. The former runs:—

Here are deposited
The remains of NEVIL MASKELYNE,
D.D., F.R.S.,
Astronomer Royal Forty-six Years.
He died on the 9th day of February, 1811,
at the Royal Observatory in
Greenwich Park
in the 79th year of his age.

The Latin inscription in the church is:—

In hujus Ædis Cœmeterio
Depositæ sunt Relliquiæ
NEVIL MASKELYNE, S.T.P., R.S.S.,
Necnon Astronomi Regii:
Viri
Seu Morum Simplicitatem
Seu Animi Benevolentiam
Seu Vitæ doctissimæ utilitatem
Spectes,
Insignis, Eximii, publice deflendi:
Qui Naturæ Leges exprimendo
Auctorem Maximum potissime coluit:
Sine fuco pius
Pietatem officio exhibuit:
Denique, non sibi, sed Christo fidens,
In gremium Patris Sempiterni,
Vitam bene actam
Futuri certa spe præmii
Reddidit
Quinto Id. Feb: Anno Domini 1811.

One almost regrets that this inscription does not contain more reference to Maskelyne's scientific work, particularly to his establishment of the 'Nautical Almanac.' It is interesting to remember that the transit of Venus in 1761 was observed by two future Astronomers Royal,—by Bliss, who supplied the place of Bradley (too ill to observe himself) at Greenwich, and by Maskelyne, who observed it (though only partially on account of the weather) at St. Helena.

Yours faithfully,
W. T. LYNN.

Blackheath, 1891, April 13.

The Transit of Mercury as seen at the Madras Observatory.

GENTLEMEN,—

The following notes on the recent transit of Mercury, as observed here, may be of some interest to your readers. Mr. Pogson being unfortunately disabled by serious illness from any active exer[illegible]n asked me to assist him on the occasion, and this I gladly did.

The telescope which I used was the Lerebours and Secretan equatorial of 6¼ inches aperture, and the power employed for observing the contacts was 102. Mrs. Pogson observed with the Troughton and Simms equatorial of 8 inches with an eyepiece of the same power.

The first contacts took place before sunrise, so that only those at egress could be observed. The early morning was cloudy, and until after 9 o'clock the Sun was only seen at intervals between clouds. For the last hour, however, the sky was fairly clear, but the air was very unsteady. The first points to which attention was directed were the central white spot and the halo which have been recorded on previous occasions. The white spot was distinctly seen by Mr. Pogson and myself, and to both of us it appeared to be absolutely central. The appearance was exactly such as one can produce by placing a small opaque disk in a beam of diverging rays, and it is hard to believe that the cause is not the same. No trace of a halo could be observed at any time during the transit. After making careful observations of the spot, I examined the spectrum by means of a direct-vision spectroscope attached to the telescope. The disk of Mercury formed a perfectly black band along the spectrum, and there was not the slightest trace of any thickening of the absorption-lines close to the planet. Probably the power used was not sufficient to show this even if Mercury had a considerable atmosphere; but the observation seemed to be worth making. The observations of the times of contact at egress were made with great difficulty on account of the unsteadiness of the air causing vigorous undulation of the limbs of both the Sun and the planet. The last internal contact was probably determined much more accurately than the last external contact; but both are recorded considerably before the times calculated from the general reduction formulæ in the 'Nautical Almanac.' There was no sign of any ligament or black drop; but just before the last external contact the part of the planet still on the Sun's disk appeared like a black triangular notch, which seemed to disappear and reappear repeatedly before Mercury was clear of the Sun. The following are the reduced Madras mean times of contact as observed and calculated:—

	Madras Mean Times.		
	Observed.		Calculated from N.A. Formulæ.
	Mrs. Pogson.	C. M. S.	
	h m s	h m s	h m s
Last Internal Contact...	.. ...	22 5 2	22 6 53
Last External Contact...	22 9 42	22 9 28	22 11 48

Mr. Pogson had his favourite Smythian telescope of $3\frac{2}{5}$ inches aperture, with a power of 95, in readiness in the open air; but wind and bad definition rendered it useless except for an occasional brief and unsteady view, and especially so just at the times of contact. It is only fair to add that counting of clock or chronometer was rendered scarcely possible to him and to Mrs. Pogson, owing to the presence of visitors, who seemed unable to understand the necessity of quietness. All three telescopes were supplied with first-surface reflecting-prisms so as to render their full apertures available with moderately dark neutral tint eyepiece shades.

There were two large groups of spots visible on the Sun's disk, besides a small circular spot which looked very like Mercury except that it was surrounded by a well-marked penumbra.

Madras, 1891, May 14. C. MICHIE SMITH.

OBSERVATORIES.

GREENWICH.—The Annual Visitation of the Royal Observatory was held, as usual, on the first Saturday in June. The weather was fine generally, though passing clouds sometimes obscured the Sun, especially during the time of the partial eclipse, which many of the visitors watched with interest. The chief objects of interest were the stellar photographs (particularly a beautiful plate of the cluster in Hercules); the new building in the South Ground intended as a museum, and a nucleus for further additions: a new transit pavilion in the Front Court; and the photographic records of earth-currents, which have recently been simply records of the new electric railway whenever it was in action, although the nearest wire is some miles from the railway.

The Report of the Astronomer Royal calls attention to the necessity for largely increased accommodation in many ways, and it is to be hoped that the wings will be speedily added to the central building in the south ground, which was designed specially in view of such necessary extensions. One of the calls for more space comes direct from the Admiralty, who wish suddenly to largely increase the number of chronometers and deck-watches for the Navy.

The regular work of the Observatory has proceeded along the usual lines. A project for determining the longitude of Montreal has been sanctioned by the Admiralty, and preparations are being made for beginning the work this year.

Under the genial presidency of Sir W. Thomson, the Visitation Dinner was this year a great success. It was held at the Criterion Restaurant, the move from Greenwich to London being deemed advisable in view of the gradually changed conditions. The greater number of those present remained till an advanced hour, with every appearance of contentment.

Paris.—In his report for 1890 Admiral Mouchez calls attention chiefly to the completion of the building for the great Equatorial coudé and to the creation of a Spectroscopic Department. The latter is at present under the direction of M. Deslandres, who is already earnestly at work. The Director then proceeds to make a powerful appeal for the removal of the Observatory, or a great part of it, to some spot outside Paris. This appeal has been made before and the scheme generally approved, but has not received sufficient support to carry it into effect. Admiral Mouchez insists that the matter is one of urgent necessity. One of the disturbing causes which make the removal necessary is of a rather novel kind, viz. the electric lighting of neighbouring streets, which makes stellar photography impossible through fogging of the plate. But there are five other good sound reasons for the change, any one of which should be sufficient.

M. Périgaud has taken over the superintendence of the Meridian Service from M. Loewy. Some changes of real importance, though trifling in appearance, have been made in instrumental methods. The large gas-jet used for illumination of the field has been reduced to insignificant dimensions, "profiting by a remark of M. Cornu." (M. Cornu is very strong on the point of reducing the size of such jets. He declares that a match-flame is several times more than is actually necessary for the largest instrument, if a proper lens be employed.) The silver circles for micrometer-drums have been replaced by celluloid. The new mercury-baths work very well, &c., &c.

M. Loewy's work progresses. His devices for observing circumpolars are receiving proper attention. The aberration-work being practically finished, an attack has been made on refraction with all its thermometric difficulties. M. Bigourdan and Mdlle. Klumpke have been active in the observations of comets and planets; and the brothers Henry have taken 48 negatives of different regions, and solved the problem of enlargement of negatives.

We are glad to hear that the second volume of the Great Paris Catalogue is nearly completed, with its accompanying volume of observed positions.

The report closes with a few words on the Montsouris Observatory, which was established for educational purposes in 1875. The land assigned to this purpose was almost immediately bought back by the Municipal Council for a park at a price of 5000 francs, and with another 2000 francs generously given by M. Bischoffsheim the original pavilions were solidly rebuilt, and since 1877 the two equatorials, two transits, and photographic appliances of the establishment have been freely open to all who wished to learn observing. Instruction is given on various matters besides actual observing, and the institution is apparently very successful.

Oxford.—The report of the Savilian Professor cantains one sentence to which we must take exception. It is stated that "from peculiar circumstances not likely to recur for a long time, the Observatory work during the year has been both varied and active." The alteration we would suggest is only slight in form, to wit, the substitution of "varied as well as active" for the last four words; but those who know Prof. Pritchard will sufficiently recognize the injustice he has done himself to prefer the emendation.

The "variety" is that of a transition stage. "The completion of the whole scheme of the determination of stellar parallax, as originally conceived, may be said to constitute the second epoch in the History of the University Observatory. The publication of the photometric work in the 'Uranometria Nova Oxoniensis' I regard as the first. In like manner the third epoch now commences with the construction of the Photographic Star Chart." Many happy returns to the 2-epoch-old youngster!

Details of the preparations for the new work occupy a considerable portion of the report. Mr. Plummer has quite obviously been "both varied and active." Prof. Pritchard's well-known affection for a wedge has led him to devise one for measuring the photographic magnitudes of stars, as already described in the 'Monthly Notices.' The financing of the new work appears to be as satisfactory as all Prof. Pritchard's similar dealings with the University. An interesting relic associated with the two Herschels has been acquired for the Library, viz. "a print of the 40-foot telescope from a negative by Sir J. Herschel, the earliest ever taken on glass, which may be said to be the foundation of modern photography."

POTSDAM.—An interesting account of the Potsdam* triad of Observatories (Astrophysics, Meteorology, and Geodesy), contributed by the respective directors, has been officially published. The history and work of each is fully detailed, and excellent plans and illustrations are added. The Astrophysical Observatory considers itself the child of the stirring events of 1870. The idea of such an observatory had been entertained for some years, but the actual establishment of it first became possible "through the new constitution of the German Empire after the glorious war of 1870 and 1871, and the consequent demands on industry and commerce, art and science."

VATICAN.—We are glad to see the first fascicule of the publications, dated 1891. It opens practically with the Papal Brief of which Father Cortie gave an account last month (No. 176, p. 226), and which constituted the Observatory. Then follow an historical introduction and two papers on Astronomical Photography, to which the observatory is to be at present devoted, the first of these being a general review of this department of Astronomy from its rudimentary stage to the present culmination in the work of the Chart, and the second an account of a visit to Paris by Father Lais. The observatory being yet in its infancy, there are but few observations (chiefly meteoric and meteorological) to record; but it is an excellent start to publish whatever there may be without delay; and there is after all a good deal in 180 pages quarto.

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* Die königlichen Observatorien für Astrophysik, Meteorologie und Geodäsie bei Potsdam. Berlin: Mayer & Müller, 1890.

San Fernando.—Capt. J. Viniegra has been appointed Director in succession to Capt. Pujazon, whose sudden death we announced some few weeks ago.

PUBLICATIONS.

West-African Longitudes.—"The longitudes of many places on the West Coast of Africa have long been in doubt. When, therefore, at the end of 1889 telegraphic connection between St. Paul de Loanda and Cape Town was on the point of being completed, the opportunity of utilizing the cable presented by the fact that Commander T. F. Pullen was to be relieved from command of H.M. surveying-vessel "Stork" at Simon's Bay was gladly seized, and it was arranged for him to visit in one of H.M. ships as many of the stations where the cable touched as was possible and were considered healthy, so that he might determine the differences of their longitude from the Cape Observatory, an object in which the Astronomer at the Cape warmly cooperated." In these words the Hydrographer introduces to us the results of the work of Commander Pullen, which cost him his life. Both the quantity and quality of that work are astonishing. On 1889, June 10, Commander Pullen arrived at the Cape Observatory, knowing scarcely anything of what was required of him. Within a few months he had mastered the subject and determined the longitudes of Port Nolloth, Mossamedes, Benguela, St. Paul de Loanda, São Thome, and Bonny; but on November 2 he died from malarial fever contracted at Bonny on October 27. And although he was only armed with a vertical circle, which seems to have had some instrumental defect only subsequently discovered, the accordance of the results points to such extraordinary care and skill on the part of the observer as to recall to Dr. Gill, who has reduced and published the observations, a remark of Prof. Winnecke, "that the best possible training for an astronomical observer is a long course of *accurate work* on land with the sextant."

The details of the operations are perhaps too technical to interest the majority of our readers, though we must pay a tribute in passing to the skill of the editor, Dr Gill, who has had to deal with a set of observations affected by that tiresome class of instrumental defect which is only discoverable from the reductions, and which seems to have been in this case a dropping of the wire frame through a few seconds of arc.

But an important sentence occurs, made in the concluding remarks, viz.: "It is worthy of note that in the determination of all these *absolute* longitudes (with the exception of Bonny) by far the largest source of probable error lies in the determination of the longitude of Aden. It would be of considerable interest therefore to redetermine the longitude of Aden with the utmost possible refinement."

NOTES.

THE PAST SUMMER IN AUSTRALIA.—By a meteorological report received from Mr. Todd (Adelaide, Australia) it appears that the past summer in Australia has exhibited some remarkable climatic conditions. With one exception, that of 1884–85, the summer just concluded has been the coolest since the Observatory records began in 1857. Mr. Todd makes a detailed comparison of these two summers, and, briefly summing up, finds that the summer of 1884–85 was very cool, rather cloudy and wet, with a mean barometric pressure about the average, whilst this year it was very cool, very clear and dry, with a high mean pressure. As to the prospects of the coming winter, it has been found, from an examination of previous records, that a "cool summer with a high barometer" is followed by a dry winter in eight out of eleven cases; but Mr. Todd hopes that the coming winter may prove an exception to the general rule.

A GOODLY company assembled at the Royal Institution on Wednesday, June 17, at 4 of the clock, to hear Lord Rayleigh lecture on Faraday. This and another lecture by Prof. Dewar were arranged to celebrate the Faraday centenary. The Prince of Wales presided, and after the lecture admitted several distinguished foreigners to honorary membership of the Institution. The published list of such admissions was a long one, but only four of the nominees were present in person to receive the portfolios containing their diplomas. In the lecture Lord Rayleigh concisely expounded Faraday's physical discoveries; and it was rather the manner than the matter of the exposition that was at all new; but one point was specially interesting. The lecturer had found in Faraday's notes a description of waves seen on rare occasions in smooth water on a flat beach, with crest parallel to the direction of the wind instead of perpendicular to it as usual. He could not account for these theoretically; and almost doubted the observation until recently, when he had been able to verify it himself; but he "still doubted whether anyone but Faraday and himself had ever noticed such waves;" and the matter was significant as attesting Faraday's wonderfully keen powers of observation.

We had already in type, as an addendum to the letter of Prof. Michie Smith on p. 267, an extract from a personal letter sent at the same time which spoke of the serious nature of Mr. Pogson's illness. Only too quickly was this followed by the news of his death, which will come to many as a great shock. Mr. Pogson has been for a long spell of years in India without returning home at all; and has not conspicuously succeeded in overcoming the peculiar difficulties of his task as Director of the Madras Observatory. But his earlier work, especially that at Oxford, has made his name familiar to all. He discovered in all 8 minor planets and 20 variable stars; but the simple suggestion for a uniform scale of star magnitudes is probably his best claim to fame. Mr. Pogson was 63 years old, and had been at Madras since 1861. He died on Tuesday, 1891 June 23.

New Method for determining Aberration.—The failure of Talcott's methods for determining aberration, owing to the alleged variation in the latitude, has led Dr. Küstner, of Berlin, to devise a new method*. The difference of declinations of two stars, deduced from meridian zenith distances on the same day, is independent of the particular value of the latitude on that day; but if the stars are separated in R.A. this difference will be affected by aberration, the factor being a multiple of $\cos\delta \sin \frac{1}{2}(\alpha_1-\alpha_2)$, where α_1 and α_2 are the right ascensions and δ the mean declination of the two stars. If, then, a series of stars be selected at nearly the same declination δ, and at R.A.'s α_1, α_2, α_3, &c.; and these be observed in pairs at a place latitude ϕ, and consecutive differences of declination formed round the cycle, the sum of these differences (which should be zero if there were no aberration) is the constant of aberration multiplied by

$$2\sin\phi\cos\delta[\sin\tfrac{1}{2}(\alpha_1-\alpha_2)+\sin\tfrac{1}{2}(\alpha_2-\alpha_3)+\ldots+\sin\tfrac{1}{2}(\alpha_n-\alpha_1)].$$

The constant of aberration can thus be determinated from the sum of these differences, and is best determined when this factor is large, *i.e.* when

(1) ϕ is large, or the place of observation is as far north as possible.

(2) The differences $\alpha_1-\alpha_2$, $\alpha_2-\alpha_3$, &c. are all equal. If there are p of them the value of each will be $\frac{2\pi}{p}$, and the factor becomes

$$2\left(p\sin\frac{\pi}{p}\right)\sin\phi\cos\delta,$$

which increases as we increase p.

When $p=2$, the value of the bracket is 2; and when p is infinite, the value is π. In practice not much is gained by increasing it beyond 3 or 4. The method thus comes to this:—Choose (say) 4 stars, or groups of stars of nearly the same declination, and of R.A.s α, $\alpha+6^h$, $\alpha+12^h$, $\alpha+18^h$. Observe declinations of the first two on the same night for as many nights as are convenient and daylight allows. Then continue with the second and third; then with the third and fourth, and finally, the fourth and first. On adding together the differences of declination the sum will be found to differ from zero, and when divided by the above factor will give the constant of aberration; and this result will be independent of any but diurnal changes in the latitude.

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* Astr. Nach. No. 3015.

Dr. Küstner considers the application of the method in practice for different instruments, and gives an example of the method from Z.D.s observed at Berlin by Dr. Marcuse, the constant of aberration coming out $20''\cdot490 \pm 0''\cdot018$. But this is only a preliminary example.

VARIABLE STARS OF LONG PERIOD.—We should like to call attention to a Harvard circular asking for cooperation in the observation of 17 circumpolar variables of long period. "Many astronomers," it runs, "provided in some cases with excellent telescopes, find difficulty in using them in such a way as will really advance astronomical science. The study of these variables seems especially adapted to such cases. Except the telescope itself, no delicate apparatus, like clock-work or micrometer, is required." The subject of investigation is the light curve of these stars. The methods to be followed are clearly and simply explained in the circular, a copy of which can be obtained from Harvard College by such as desire to take part in the work. "All persons observing the variable or comparison stars according to the system described above are invited to send their results to Cambridge (Mass.) for reduction and publication on same system as our own observations. . . . Should this work commend itself to astronomers, it is hoped to extend it to the other variables of long period."

DOUBLE STARS.—In the 'Astronomical Journal,' No. 241, Prof. Glasenapp gives some measures made in 1890 of fourteen stars noted as double in the 'Uranometria Argentina.' With two exceptions, which are also in Mr. Burnham's Catalogue (β 347 and β 639), these are the first published measures, as no measures were made at Cordoba. The observations were made at Gourzouff in the Crimea with a 6-inch refractor, and form really only a small portion of measures of 400 doubles made at the same time. Can we have the remainder?

TRANSIT OF MERCURY, 1891, MAY 9–10.—*Adelaide* (Mr. C. Todd, C.M.G.).—The ingress was observed here under very favourable conditions, the Sun's limb and the projection of the planet on the Sun's disk being very sharply defined. At egress, however, the definition was bad and observation of contact difficult. The phenomenon of the black drop was very distinct. A careful watch was kept for the planet, but nothing of it was seen after it passed off the Sun's disk. Mr. Todd was one of the observers who saw Venus plainly after the transit of 1874. There was at times a suspicion of a faint shading round the black disk of the planet during transit. Mr. Todd also thinks he saw some greyish markings on the planet itself, but says the impression was very doubtful and only mentioned for comparison with other observers.

Göttingen (Schur).—The observations here ought to possess some value, as no less than eight observers successfully noted the contacts.

Breslau (Galle).—Here also the transit was observed by three observers under favourable circumstances.

Christiania (Geelmuyden).—Two observers noted the times of contact.

Heidelberg (Wolf); *Gern* (Krieger).—At both Observatories the definition was very good.

Dresden (Engelhardt).—Images unsteady. Observation not worth much. The disk of planet was noted as much darker than the neighbouring sun-spot.

In all the above the black drop was distinct and several times observed; at Jena no drop was seen.

THE MERIDIAN OF GREENWICH.—M. Tondini de Quarenghi thinks the meridian of Greenwich is unsuited to be a universal meridian because the climate is so bad. He says so in a note to the French Academy *; and that of Jerusalem cuts the equator, the two tropics, the circle 45° N., and the arctic circles on dry land. We are anxious that the question should be considered on all its merits, and are glad to help M. Tondini by stating these facts, though we are not responsible for pointing out their importance.

PROBABLE AVERAGE PARALLAX.—Dr. Kleiber, referring to the well-known fact that a comparison of the spectroscopic results for motion in line of sight with the proper motions in arc will give a probable parallax, finds from the Potsdam spectroscopic results an average parallax of $0''\cdot07$ for stars of the 1st and 2nd magnitudes. He has previously shown how to estimate this parallax in another way by counting the number of stars already discovered (see No. 162, p. 185), and by this method finds $0''\cdot12$, which he considers disappointingly different from the former result. We are rather inclined to think the accordance very good for two such methods.

IT is always pleasant to hear of new developments of astronomical activity; and though the Astronomical and Physical Society of Toronto is now more than a year old, the annual volume of 40 pages quarto which we have just received is the first serious evidence of its birth. The meetings have evidently been successful. Not attempting too high a flight at first, the few members present have read together an interesting paper from some astronomical publication, or one of them has given an account of some important piece of work. But the Society already shows signs of passing out of this early stage, and we heartily wish it a prosperous career.

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* C. R. cxii. p. 1189.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 178. AUGUST. 1891.

Notes on Observations of Meteors.

At about the end of the third week in July the meteoric season may be said to commence, for twilight is then leaving the midnight sky and the Perseids begin to manifest themselves in force. It is at this period that a great rise becomes apparent in the visible number of meteors, for many Aquarids as well as Perseids are usually seen, and especially so at the close of July. The meteoric observer should therefore trim his lamp in preparation for renewed vigils and thoroughly utilize a season which offers every inducement to successful and agreeable observation. The nights are warm and often clear. Meteors are uncommonly plentiful. Several of the most interesting streams are displayed, and on the whole the prospect is one which guarantees an abundance of results to the active observer.

The work of recording meteors accurately is one needing considerable practice before desirable proficiency can be attained. An observer coming fresh to the work is apt to be confused at the suddenness of the phenomena and his earliest impressions are rarely to be depended upon. He is not accustomed to deal with such fugitive objects, and is certain to find that they exhibit a very erratic and untractable disposition. Just when he turns to the east he catches an imperfect glance of a meteor falling in the south, and then he decides to watch the latter region only to be again deluded, as a bright meteoric flash comes from the point he was watching before! It is very rarely that an observer is looking directly towards the exact place of a meteor's apparition. He generally has to turn a little to catch the object, and is almost sure to feel perplexed and uncertain in estimating the direction of flight. But with more experience he gains confidence and is soon able to acquire a fair degree of precision. His observations accumulate rapidly and are found to indicate a series of radiant-points which he carefully tabulates and compares with previous records. The interest in his work then increases as he finds that several of his newly-determined radiant-points corroborate showers already recognized by observers, while several others are probably new and their activity only recently developed.

To stand and gaze skywards, simply recording the meteors as they appear, is not in itself a proceeding that maintains interest; it is rather the discussion of results and comparisons that encourage the observer to renewed effort. Every shooting-star has a significance and a history, and it is the observer's duty to learn as much of these as is practicable from the evidence of his eyes.

Observations are best conducted from a garden, for an open window permits only a limited view of the firmament. A position out of doors enables the observer to glance to any region of the sky and should a large meteor appear he will be able to record it even should it fall in a part not comprised at the moment within the field of observation. The light of such an object will cause him to turn round in time to catch the end of the path. In very cold weather an open situation is uncomfortable, but its advantages are obvious.

The observer must have his star-charts or celestial globe near him so that the meteors may be quickly registered. A bull's-eye lantern will also be required, but its direct light must be kept from entering the eye, for if the pupil becomes contracted it is impossible to discern small meteors. A chair with the back suitably inclined is also a desideratim; but on a warm night, if meteors are rare and the observer not very actively employed, his comfortable position induces sleepiness and he has as much as he can do to keep his eyes open. A convenient way of observation is to lean against a horizontal bar, about 3½ feet from the ground, as this prevents the fatigue that would be occasioned by standing for some hours and allows the observer to assume a posture which does not bring on cramp in the muscles of the neck.

Some contrivance is needed to assist eye-estimates, and this is found in a straight wand which the observer should hold in his hand and project upon the path of each meteor directly it is seen. He may then note its direction accurately and reproduce the path on his chart or globe. When the special object of the observer is to determine radiant-points, the exact places of beginning and end of flight are not such important features to observe as the *direction*; but when the work is undertaken with a view to secure double observations and compute the heights and real paths of meteors, then it is most essential to be precise as to the places of appearance and disappearance.

One of the most difficult elements in meteoric observation is to estimate the *durations* of flight correctly. In the case of very swift meteors this is impossible, as the time of visibility is less than one second, and in many instances the apparition is simply an instantaneous flash having no sensible duration. With regard to slow meteors approximate values can be ascertained by counting at a moderate pace and then, when the meteor is gone, repeating the process and deriving the interval occupied by comparison with a watch. I managed to acquire tolerable proficiency in this work by getting a friend to discharge arrows from a bow while I estimated their times of flight and simultaneously determined them by means of a stop-watch. Practice at this will enable anyone to secure accurate results for slow meteors, for when the duration is 3 or 4 seconds the error of estimation need not exceed 0·3 second. It is advisable under any circumstances to give the durations of all meteors recorded, even though the observer is conscious that the values are affected by large probable errors.

The particular features of each meteor should be noted, for this is the "divining rod" from which their radiants may be ascertained. Streaks, trains, crooked paths, swift or slow motions, long or short courses, &c., &c., are criteria of the utmost significance and must be registered for every object. The observer, in sifting his results, will then be able to allot the meteors to their radiants with some degree of confidence, and he will find that every shower has distinguishing features by which it may be recognized.

The discursive motions of meteors on ordinary nights of the year have often occasioned surprise. Their lines of flight are not consistent with two or three centres of emanation; indeed, their directions are so various that a spectator may well fancy them to be scattered broadcast over the firmament and quite uncontrolled by any law of radiation from definite points. This arises from two causes, namely, the large number of streams in simultaneous play and their extreme tenuity. There are some scores of showers in action on every night of the year, and many of these do not discharge more than one meteor in 7, 8, or 10 hours in the sphere of vision commanded by a single observer! These facts show the necessity for prolonged and accurate observation and for close attention to all those details which alone can enable the observer to discriminate between real and false radiants and to single out the trustworthy positions.

The great abundance of feeble showers renders it imperative that observations during several nights should be combined, for by this means only is it possible to locate the radiant-points with safety. It may be thought that a display yielding only one meteor in about 8 hours is incapable of being determined, but this is not the case: an observer of experience will find it practicable by watching all through the night, by undeviating precision in recording the directions of flight, and by fully considering all those appearances which assist in the identification of the radiants. It is very interesting, though critical, work watching for these exhausted streams, for a really difficult object gives zest to the pursuit, and the observer is often more gratified with a success of this kind than with one more easy of attainment.

The paths of meteors seen should be registered in a book ruled for the purpose and the place of the assumed radiant appended to each one. Then when the observations have been continued for several years the observer may institute comparisons amongst those seen on nearly the same dates in each year and he will probably detect some tenuous showers which were too weak to fully manifest themselves in the results of a single year.

A record should be kept of the number of meteors visible during each hour of observation. This will ultimately enable the observer to deduce the horary averages for the different months, and to find the relative numbers for the different hours of the night.

In any comparisons with other observations or in any investigation or theory depending upon the radiants of various observers extreme caution must be exercised. It has been truly said that "figures may be made to prove anything;" and this is forcibly applicable to discussions based on the radiant-points of meteors. If anyone has an idea with respect to the grouping of radiants, to stationary or shifting radiants, or to their duration or diffusion, he may, from the mass of incongruous results of past observers, easily obtain the figures to suit his views. The whole subject is complicated in an extreme degree, and I would advise the observer in his pursuit of truth to work independently of all previous results. After-comparisons will, of course, be useful, but his observations should be absolutely free from bias and untrammelled by any attempts to make them amenable to theory.

Much has been accomplished in past years, but meteoric astronomy is one of the youngest branches of the science, and is far from being developed in a thorough manner. As in the department of comets, so in this, there are some prominent facts of observations which have yet to be explained. Diligent observers in the future will doubtless accumulate evidence that will enable present doubts to be cleared away. Among the points requiring further attention I may mention the following:—

1. *Stationary Radiants.*—Do meteors radiate from identical points in the heavens for several months in succession? What are the differences of motion shown by the meteors from these stationary radiants (if existing) as their positions change relatively to the earth's apex?

2. *Moving Radiants.*—The fact of displacement in a radiant is proved by the August *Perseids.* What are the positions of this shower between July 8 and 18 and August 15 and 20, if visible during those epochs? The direction and rate of the displacement (suspected) in the Lyrid radiant (April 18–21) requires further observation. The other periodical displays should be closely watched during their activity for traces of change.

3. *Diffuse Radiants.*—To what degree are radiants diffuse? Is this diffusion real or chiefly to be ascribed to observational errors and to the allotment of meteors to their wrong centres? In cases of obvious diffusion (such as the Andromedes of Nov. 27) is the area round or elongated?

4. *Meteor Heights.*—In certain instances of doubly observed meteors their beginning heights have been found to exceed 100 miles. Further reliable cases of this will be interesting.

5. *Andromedes.*—Do the meteors of Biela's comet (Nov. 27) form a complete ellipse and become visible every year like the Leonids, Perseids, &c.? Is the shower widening with time? Its observed duration in 1885 was much greater than in 1872.

6. *May Aquarids.*—Is the radiant-point of this display at 326° −2° (Tupman) or 337° −2° (Denning)?

7. *Number of Meteors.*—At what time of the night is the maximum of meteors observed, and does the hour vary with the season?

The elucidation of these and of other points that might be suggested will, no doubt, receive proper attention in future years. The utility of storing up observations of many hundreds of feeble showers is perhaps questionable. But we certainly need to learn more of the behaviour of the known systems, and to watch these narrowly, tracing their vagaries to just causes. This will furnish ample work to those who enter this delightful field of observation.

Bristol, May 16, 1891. W. F. Denning.

The Measure of the 52nd Parallel in Europe *.

The measure of the 52nd parallel in Europe, between Valencia in Ireland and Orsk in Russia, on the borders of Asia, was proposed in 1861 by W. Struve and Baeyer, and approved by both Belgian and English astronomers and geodesists. Of all arcs parallel to the equator and traversing our part of the world, it is certainly the longest ($69\frac{1}{2}°$); and it is also fortunately very easy to measure, for it crosses countries with very few mountains. But for the completion of this great work it was necessary, not only to laboriously reduce the geodetic triangulations already made in 1861, but to undertake new triangulations, and to connect them with those previously made, and this by methods not always the same. The greatest part of the work fell on the Russian geodesists; and their work has quite recently culminated in the publication, in two quarto volumes †, of the report on their geodetic operations and reductions for the measure of the Russian portion (39° 47′) of the arc. In presenting this work and the general map of the triangulations between Valencia and Orsk to the Academy, I have the honour to make the following remarks in explanation:—

1. The Map represents, as I have mentioned, all the triangulations in existence along the 52nd parallel in Europe, which have served as a basis for the calculation of the arc in question. But

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* Translated from 'Comptes Rendus,' cxii. No. 10, p. 512.

† Mémoires de la Section topographique de l'état major général russe, Vols. 46 et 47. Publiés sous la direction du Général Stebnitsky: St. Pétersbourg, 1891.

these calculations are only complete from Haverfordwest in Wales to Nieuport in Belgium, and again from Tchenstokhov in Poland to Orsk. There are thus two lacunæ to fill in this great international work—the first from Haverfordwest to Valencia; the second from Nieuport to Tchenstokhov. The length (in degrees) definitely measured is thus not yet 69½°, but only 47¼° (7¾° in the English part and 39½° in the Russian); 22° remains to be measured.

2. The results of the English operations between Haverfordwest and Nieuport were published some time ago (1867) by Capt. Clarke; but the Irish network, to Valencia, is still to be reduced.

3. The German and Belgian network and reductions are not yet complete, for the German geodesists wish to publish them in German, which will be done under M. Helmert's direction.

4. The Russian part of the 52nd parallel whose length, east of Tchenstokhov, is definitely established contains 39° 24′ of longitude, and has no break. To determine the length in metres, 364 spherical triangles have been calculated, 114 of which belong to a control triangulation made by M. Jilinsky.

5. The numerical results of the measure of the Russian arc (Tchenstokhov—Orsk) and of the Anglo-Belgian arc (Haverfordwest—Nieuport) are collected in the following Table * :—

Chief stations of the réseaus and arcs.	Geodetic difference of longitude. =L.	Astronomical difference. =*l*.	*l*—L.	Length of arc in metres.
A. *Russian Arc.*	° ′ ″	° ′ ″		
1. Tchenstokhov—Varsovie ...	1 53 57·77	1 54 8·85	+11·08	131854·1
2. Varsovie—Grodno	2 48 10·12	2 48 3·45	− 6·67	192501·4
3. Grodno—Bobrouisk	5 33 38·38	5 33 46·50	+ 8·12	370462·1
4. Bobrouisk—Orel..............	6 50 14·77	6 50 23·70	+ 8·93	469605·9
5. Orel—Lipetzk	3 22 24·02	3 22 18·15	− 5·87	243027·2
6. Lipetzk—Saratov	6 26 12·99	6 26 25·35	+12·36	441906·5
7. Saratov—Samara	4 2 34·94	4 2 21·60	−13·34	277521·1
8. Samara—Orenbourg	5 1 27·02	5 1 35·85	+ 8 83	344917·6
9. Orenbourg—Orsk	3 27 23·22	3 26 47·70	−35·52	237290·8
Total for Russia.........	39 26 3·23	39 25 51·15		2709086·7
B. *English Arc.*				
1. Nieuport—Greenwich	2 45 30·71	2 45 25·20	− 5·51	189460·1
2. Greenwich—Haverfordwest	4 57 44·33	4 57 48·60	+ 4·27	340819·4
Total for England......	7 43 15·04	7 43 13·80		530279·5

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* There are obviously several misprints in the Table as given in the C. R.: those in columns L, *l*, and *l*—L have been conjecturally altered; but the total for last column (Russia) is given as 2709132·8, and if this is correct there is no internal evidence to show which of the items is in error.—H. H. T.

These figures give for *mean* length of a degree of longitude on the 52nd parallel

In Russia.... $68^{\mathrm{km}}\cdot6412$,
In England .. $68^{\mathrm{km}}\cdot6880$,

which proves that the length is not the same all over Europe. We arrive at the same conclusion by considering different parts of the Russian arc itself; *e. g.*

Varsovie to Grodno	$68^{\mathrm{km}}\cdot7662$,
Samara to Orenbourg	$68^{\mathrm{km}}\cdot6556$,

whence it results that the 52nd parallel, in Europe, is not a circle but a nondescript curve approximately circular only; or the Earth's surface is so far not an ellipsoid of revolution.

The accuracy of the above figures on which this conclusion depends seems to be guaranteed, for—

a. The *geographical positions* of the fundamental points of each trigonometrical network have always been determined, at any rate in Russia, with good instruments, and observations repeated *six* times; the *longitudes* are telegraphic.

b. The *geodetic operations* have been controlled in Eastern Russia, by an independent measure, by a single observer, who did not take part in the original work.

It thus appears that the 52nd parallel is not a circle; and a similar result was found for the 42nd parallel in the United States. Consequently, *the Earth is not a perfect spheroid.*

VÉNUKOFF.

Selenographical Notes.

SUNRISE ON SCHICKARD.—A very moderate change in the angle of illumination at sunrise makes a great difference in the appearance of the western wall of this formation,—to such an extent that much of the curious and complex detail revealed before a ray of light reaches the interior is in a great measure untraceable when the loftier portions of the opposite border are in sunshine.

At 8^{h} 45^{m} on April 20, this year, the western wall was on the terminator, and no part of the floor illuminated. Under a power of 350 it was a beautiful object, exhibiting a bewildering number of passes, depressions, craters, valleys (mostly intersecting the border obliquely), and of hills with long spurs running out to nothing on the surface of the neighbouring plain. The valleys are mostly shallow, and the hills of no great altitude. There appears to be a complete absence of concentric mountain-ranges, *i. e.* of ranges parallel to the contour of the formation, the border consisting principally of short hill-sections presenting a convex front to the interior, and ultimately trending towards the south-west in the southern half, and to the north-west in the northern half of the wall. This succession of curved inflexions is well shown by Schmidt and Mädler, but no drawing, however carefully executed, can give more than a general idea of the intricacy of the circumvallation or of the detail displayed during the early stage of sunrise.

At about 9^h the first rays of sunlight reached the floor over a low section of the south-west wall, illuminating a small rectangular area, apparently a little north of the ring-plain *a* in Neison's Map XVI. A bright little crater, casting a long spire of shadow, was noted on the western side of this area, and in the midst of the darkness, about midway between this and the south-east wall (which was just coming into sunlight), another somewhat larger crater was visible, which, though not traceable in the maps, appears to stand very close to *a*. As sunrise advanced two isolated oval-shaped patches of the floor immediately north of the rectangular area came into view, evidently representing slightly elevated districts thereon. I was not able to watch the progress of the illumination of the interior after 9^h 30^m, but if this was carefully done from first to last the position and character of many unrecorded objects (especially craterlets) and of doubtful features might be easily determined. The interior of Schickard abounds in interesting detail of every description, much of it well within the grasp of a good 4-in. achromatic, if examined during a suitable phase. The region on the eastern side of the floor includes numerous rills, crater-rows, ridges, &c., which, under a low morning Sun and good conditions of libration and definition, would form a most useful and interesting study. I devoted some time to these details during the year 1884, and published a drawing of my observations made during the September lunation of that year, which will be found in the 'Journal of the Liverpool Astronomical Society' for December 1888.

In the country north-west of Schickard is a group of conspicuous ring-plains, Phocylides *d*, G, *h*, *i*, &c., which are well worthy of examination. The largest, *d*, has, I believe, a central mountain which has been apparently overlooked, possibly because it is only visible under a low Sun. East of this is a curious C-shaped formation, resembling an incomplete ring, lacking a border on the east. The agreement between the maps of Mädler and Schmidt in this region, even as regards minute objects, is very striking.

The crater shown only by Schmidt on the south-east wall of Drebbel was indicated on April 20th by a distinct notch in the rim of this formation.

Beaumont House, Shakespeare Road,
Bedford, 1891, July 18.

THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Assyrian Eclipses.

GENTLEMEN,—

It is somewhat remarkable that in none of the ancient Egyptian inscriptions which have hitherto been examined is any record to be found of an actual eclipse of either the Sun or Moon, although obscure allusions to the fact of such phenomena taking place have been, as Mr. Renouf has pointed out, in several cases met with. It is well known, indeed, that Brugsch thought, notwithstanding the objections of Chabas, that in an inscription made at Thebes in the reign of Thakelath (Tiglath) II., of the twenty-second dynasty of Egyptian kings, mention is made of an eclipse of the Moon. I referred to this matter in the number of the 'Observatory' for April 1888 (vol. xi. p. 197) and pointed out that no eclipse occurred at the time mentioned. Mr. Renouf has since informed me that the inscription, rightly interpreted, so far from stating that an eclipse took place, states in fact the very reverse. It is scarcely worth while to refer to George Smith's theory that the eclipse of April 4, B.C. 945, is referred to in this inscription, since its true date is nearly two hundred years after this time.

But two, if not three, eclipses (of the Sun) do really appear to be recorded in the Assyrian inscriptions. The first of these occurred in B.C. 763 on the 15th of June, and was total in or near Nineveh. There appears no reason to doubt that it is mentioned in the Assyrian so-called 'Eponym Canon,' which was interpreted by Sir Henry Rawlinson in 1862, from the fragments of terra-cotta tablets brought over by Sir A. H. Layard and placed in the British Museum. The inscription in question states (as kindly translated for me by Mr. T. G. Pinches, of the British Museum) that in the "Eponymy of Bur-sagale, governor of Gozan, a revolt in Assur [the city] took place in the month Sivan and the Sun was eclipsed." This, according to the Canon, was in the eighth year of the reign of Assur-day-an, and the record must be allowed to fix subsequent dates in the Canon with a great precision. For although Oppert, by assuming a lacuna of 46 years in it, endeavoured to identify the eclipse with the annular one of June 13, B.C. 809, there seems little ground for this or doubt that the view of George Smith is correct, and the eclipse the total one of June 15, B.C. 763.

An eclipse is also mentioned in an Assyrian tablet in the British Museum, which would seem to have occurred in the reign of Esarhaddon. Mr. Pinches thus translates the portions referring to the eclipse :—" Since the king my lord went to Egypt, an eclipse has taken place in the month Tammuz [corresponding nearly to our own June]......When I brought the account of the eclipse of the month Tammuz, I sent it away to the presence of the king." It is very difficult to identify this eclipse with any resulting from calculation; the only conjecture I can make is that it may have been one which occurred on the 27th of May, B.C. 699, and was annular in India.

The immediate successor of Esarhaddon was Assur-bani-pal. An inscription made in his reign was interpreted by Fox Talbot (see the Transactions of the Society of Biblical Archæology, vol. i. pp. 13 and 348) to record the occurrence of an eclipse of the Sun, which, at the suggestion of Oppert, was supposed to have been that of the 27th of June, B.C. 661. The inscription, as translated by Talbot, states that it took place whilst the king of Elam was preparing an attack upon Assyria, and that "for three days the evening Sun was darkened as on that day." No eclipse of course could produce such an effect as this; but he suggests that "it is not impossible that, in a very ignorant age, the report of such a wonder having happened in Susiana should be believed in Assyria, at the distance of many hundred miles, and have been chronicled by a superstitious scribe." Revising, however, a record by a conjectural process of this kind is always a hazardous proceeding, and on reading it I felt doubtful whether an eclipse was really referred to in the inscription. But Mr. Pinches has kindly examined it again, and considers that the eclipse and the three days' darkness at evening are distinct occurrences, forebodings of evil to the king of Elam. He thus, in fact, translates the passage:—"Te-umman devised evil, and Sin [the moon-god] devised against him forebodings of evil. In Tammuz an eclipse at evening—he troubled the lord of light and the setting sun thus also for three days was troubled—it went forth for the end of the reign of the [king] of Elam. This [omen] was the announcement of his [*i. e.* the god's] decision, which changeth not." Upon the whole, therefore, we may fairly conclude that the eclipse of June 27, B.C. 661, is really alluded to in this inscription.

If I am not pressing unduly upon your valuable space, I should like to state in conclusion that I have not succeeded in finding the reference to the eclipse of B.C. 776 (the first year of the first Olympiad), said, in 'L'Art de vérifier les Dates,' to have been recorded by Confucius in the Tchun-Isicou [Ch'un Is'ew], written about two hundred years after that time. I have consulted both the 'Observations' of Father Souciet, referred to in 'L'Art de vérifier,' and Dr. Legge's translation of the Ch'un Is'ew itself, in which it appears that the earliest eclipse mentioned is that which, according to our reckoning, took place on the 14th of February in the year B.C. 719. This agrees with the list given by our late Assistant Secretary, Mr. Williams, in the 24th volume of the 'Monthly Notices.'

Perhaps some one more versed in these matters will supply the reference to the eclipse of B.C. 776. Mr. Johnson, I need hardly remark, mentions it in his well-known work (of which many of us wish for a new edition) 'Eclipses Past and Future.'

Yours faithfully,
W. T. LYNN.

Blackheath, 1891, June 12.

Bern Time.

GENTLEMEN,—

The list of observatories in the 'Nautical Almanac' seems very incomplete. I do not know whether it is owing to Switzerland being an inland country that there is no mention of Bern; but it seems strange, considering the 'Nautical Almanac' is not used only by sailors. It is singular that I cannot find any book that gives the longitude exactly, without which it is impossible to ascertain the Greenwich time of a country which is one of the best in the world for its time system.

Yours faithfully,
T. W. BACKHOUSE.

West Hendon House, Sunderland,
1891, July 24.

OBSERVATORIES.

CINCINNATI.—Measures of the positions of 105 nebulæ, and charts of the region surrounding 96 of them, are nicely printed in a small quarto publication of 50 pages. The measures were made with the 11-inch refractor in 1884, 1885, and 1886, and are for the most part of objects south of the equator, many so low as to be beyond the reach of European observers. The position of each nebula with respect to one or more neighbouring stars was determined with a filar micrometer, and a list of accurate positions of the comparison stars is given. But the information printed is surely somewhat meagre. For instance, in quoting the positions of these stars for 1890·0 as deduced from various catalogues, why not give the reference number in such catalogues? It would not involve much more printing, and might save some trouble to anyone using the work.

HONGKONG.—The sixth volume of observations prepared by Dr. W. Doberck, the MS. of which is dated 1890 April 24, has been published; and shows that the meteorological observations and time-service have been maintained with efficiency. In the introduction the Director asks for another European as chief assistant, and Mr. J. I. Plummer has since been appointed to this newly created post.

YALE.—The report of this Observatory is practically that of Dr. Elkin, the "Astronomer in charge of the heliometer." He has completed the work on parallaxes of the first-magnitude stars, which will shortly be ready for publication. A triangulation of the Victoria Comparison Stars (1889) has been made by Mr. Chase and partly reduced; and progress has been made with a triangulation of Coma Berenices.

The reductions of the triangulations of Iris Comparison Stars (1888) and of the pole are also nearly completed, the former being chiefly the work of Miss M. Palmer. The heliometer is now to be devoted to measurements of the Satellites of Jupiter, according to Dr. H. Struve's method for those of Saturn.

Washington.—The volume of observations for 1885 contains, first, the regular meridian observations of Sun, planets, and stars (the annual catalogue containing 1063 stars); secondly, observations of 130 double stars and of the satellites of Saturn with the 26-inch equatorial (Prof. A. Hall); thirdly, micrometer observations of 17 small planets and 6 comets with the 9·6-inch; and, fourthly, three appendices important enough to be noticed elsewhere, viz., Lieut. Winterhalter's "Report on the Astrophotographic Congress;" Prof. Hall's monograph on "Saturn and its Ring;" and Prof. Harkness's work on the "Solar Parallax and Related Constants" The whole forms a noble volume, and as evidence of the health and vigour of the Observatory is distinctly reassuring in view of the laments we have recently noticed in the reports of the Superintendent as to insufficiency of staff.

We have also received Appendix 1 to the volume for 1886, containing magnetic observations made at the observatory in 1888 and 1889 by Ensign J. A. Hoogewerff.

NOTES.

A Photochronograph.—At the Georgetown College Observatory some fairly successful experiments have been made by Prof. Bigelow, Mr. Saegmuller, and the Director, Father Hagen, on the application of photography to recording star-transits. A transit instrument arranged for this purpose has been dubbed "a photochronograph," and the methods used are described in a quarto publication of 36 pages. The experiments are still proceeding. It is justly remarked that this method should be very valuable in longitude work, rendering an interchange of observers unnecessary; though it may still be advisable to interchange instruments.

The Stellar Cluster ψ Persei.—The two groups forming this cluster have from time to time occupied the attention of observers. Lamont, Liapunow, Bredichin, Krueger, and Vogel have all measured portions of the group with a view to investigate relative motion. The present volume by Mr. Pihl, of Christiania, forms the most complete monograph of the group, and has been formed with great care. Mr. Pihl, who, as a business man, has a limited time at his disposal, suffered for many years from ill-health, and this explains the rather long period (20 years) over which the observations extend. He must in all have made some 4000 measures of the 236 stars in the group. These 236 he has catalogued, epoch 1870·0, and compared, where possible, with other catalogues. After the publication of Vogel's measures Mr. Pihl made a comparison of the two sets and found a very marked systematic difference, which he eventually traced to the method of observing. Vogel's measures were made with a wire micrometer, the time of bisection being noted, and the star disappears, if at all, only momentarily. Mr. Pihl used a bar-micrometer, the times of disappearance and reappearance being noted. A very complete discussion of the errors to which the bar-micrometer gives rise is given. A chart of the cluster is appended. T. L.

Die Jährliche Parallaxe des Sterns Oeltzen 11677.—Since 1883 Dr. Franz has made a series of observations with the Konigsberg heliometer (6-inch) with the view of determining the parallax of this star, whose proper motion on a great circle is 3″, which places it about 12th on the list. The parallax has already been determined with differences of R.A. and N.P.D. of transits by Dr. Geelmuyden, whose results are respectively $\pi = 0''\cdot 27 \pm 0''\cdot 04$ and $0''\cdot 24 \pm 0\cdot 04$. Dr. Franz gets $\pi = 0''\cdot 10 \pm 0''\cdot 01$.

Artificial Production of Rain.—Many meteorologists have found it an attractive pursuit to endeavour, by the study of past records, to predict the future weather; but with the best means at command such prediction, even for a following day, cannot be said to have attained to a more than qualified success, whilst for longer periods nothing with any certainty whatever can at present be foretold. But if we are unable to predict the weather, it would seem in the minds of some that there is no reason why we should not control it. Our readers will have heard more or less, no doubt, of the possible artificial production of rain by the firing of cannon. It is not a new idea, but some steps appear to be now about to be taken with the object of bringing the question to a practical test. We read in the 'American Meteorological Journal' that Senator Farwell has carried through the United States Congress a bill appropriating a sum of money to be applied to this object, the experiments being entrusted to the Agricultural Department. Mr. Fernow, Chief of the Division of Forestry, in a comprehensive report has given an account of what is known on the matter, from which it would appear from the evidence of past cannonades, great fires, &c., that they afford a very overwhelming proof of rain having been by their influence caused to fall. Moisture may be precipitated if present in the atmosphere, but rain may be most wanted under clear skies, where the drought is most severe. If, however, our friends across the Atlantic proceed to carry out the proposed experiments, we ought, in the course of time, to obtain some definite increase to our knowledge on this subject.

Double Stars.—Mr. Burnham has a long series of measures of double stars in the 'Ast. Nach.' 3047–48, representing his work in this department during the year 1890. No one can read Mr.

Burnham's remarks without noticing how thoroughly well pleased he is with the great 36-inch telescope and its equipment. This is pleasant knowledge, as the measurement of the class of doubles undertaken by Mr. Burnham must be a most severe test alike of the glass, the clock, the micrometer, the illumination,—and of the man. Besides the re-measures of known doubles there is a list of 70 new ones. The present list of measures gives us an opportunity to make a remark on the working catalogues of double-star observers. Mr. Burnham says—"In the selection of stars for re-observation I have endeavoured to take such pairs as could not, or would not, be observed elsewhere, leaving the old binaries and other easy pairs to be looked after by other observers." Mr. Burnham, besides being an observer of double stars, gives a glance to the motion indicated, and doubtless finds, as we have done, that a certain half-hundred favoured stars monopolize all the attention, and a search for measures of an interesting couple is vain. We are speaking of published measures. There may be some stored away. May we ask that the following may be kept on the list? They are old, very old, but measures are scarce. Σ 13, 208, 259, 460, 511, 1093, 1216, 1687, 1879, 2315, 2525, 2729; OΣ 4, 12, 20, 62, 65, 82, 338, 395, and 400. T. L.

Dr. Gould recently stated that the Mt. Hamilton observation of Wolf's Comet (1884, III.) on May 3 gave one perihelion date by the R.A. and another by the declination. Mr. Hind, however, on comparing the place with Berberich's orbit for 1891, finds

$$\Delta\lambda\,(C-O) = -0'.1, \qquad \Delta\beta = +0'.2,$$

thus differing from Dr. Gould's result.

Mr. Hind also points out that Mr. Lynn was in error in giving Tempel's Comet of 1867 as due at perihelion this year; he makes the perihelion passage 1892 March 29·5.

The Rotation of Venus.—M. Niesten, of the Brussels Observatory, respectfully begs to differ from the conclusions of M. Schiaparelli as to the slow rotation of Venus. He finds that a period of 23 hours suits drawings made by himself and M. Stuyvaert from 1881 to 1890, so well, indeed, that he can make a map of the surface of the planet; though at the same time a period of something over 200 days gives results quite within the limits of error of such observations. The subject is one of great difficulty, and there are very few of sufficient experience to discuss it adequately. MM. Terby and Folie, to whom the present paper was referred, express themselves with admirable discretion, and manage to disagree upon one point between themselves in order to illustrate the present divided state of opinion among experts.

SKY TESTS FOR STELLAR PHOTOGRAPHY.—Dr. N. C. Dunér comments * on the figures given by the Astronomer Royal (M. N. li. no. 5) in his proposal to use short exposures with a small telescope on Polaris as tests of the quality of the night. Mr. Christie states that "a faint image of *Polaris* is obtained with an exposure of 2^s with the Dallmeyer 4-in. telescope," with rapid development "(say 1^m with the standard developer)." Dr. Dunér remarks that, by developing for 6^m instead of 1^m, an image of Polaris can be got from an exposure of 1^s with a smaller telescope. But he seems to have misapprehended the point, which is firstly to complete the whole operation quickly, so as to obtain information within a few minutes (whence it is obvious that increasing the time of development by minutes in order to reduce that of exposure by seconds is not economy); and, secondly, to use so long an exposure that there is a fair chance of estimating the *average* quality of the night.

ECLIPSES OF SATURN'S SATELLITES.—Prof. C. A. Young writes to the 'Sidereal Messenger' (No. 95, p. 251):—"On the evening of Wednesday, April 8, while looking at Saturn with a friend, we noticed that Rhea was missing, and on examining the ephemeris found that the satellite must be either behind the planet or else immersed in its shadow.

"At 8^h $56^m{\cdot}0$ [Eastern Standard Time] she reappeared rather suddenly, about 3″ from the planet's eastern limb, and opposite the parallel of 60° or 70° of south latitude. At first the satellite was very faint, but it rapidly brightened, and in about three and a half or four minutes had recovered its normal brilliance; this would indicate for it a diameter of from twelve to fifteen hundred miles. The eyepiece was an achromatic giving a power of about 500."

THE ninetieth birthday of Sir G. B. Airy (1891 July 27) is an event of such general interest that we feel sure a brief reference to its celebration by his family and friends will not be deemed an impertinence. Saturday, July 25, was fixed for the reception in honour of the event, and Sir George received in person the congratulations and good wishes of a large and distinguished company.

Astronomy was represented by many familiar faces, including those of the Astronomer Royal, the President of the R.A.S., and the Hydrographer to the Navy. It was delightful also to see Mr. Perigal, who walked up the hill with almost a jaunty step. Trinity College, Cambridge, of which Sir George is both the oldest ex-Fellow and the oldest Honorary Fellow, was represented by its Master, Dr. Butler. There were many other guests whose presence was significant, as, for instance, Mr. Biddell, who was forty years ago charged by Messrs. Ransomes and May with the construction of the present Transit Circle. He described to a small knot of most interested guests the dismay of the workmen and their employers at the demands of Sir G. B. Airy, especially those relating to the pivots. These were to be of chilled iron, 6 inches in diameter, and perfect cylinders to within $\frac{1}{30,000}$ inch! No error of this magnitude was to be discernible with a delicate spirit-level; and after trying all the most delicate methods of turning then known, the requisite accuracy was obtained by sheer labour—rubbing down bit by bit all the places which this same spirit-level indicated as too high. Each of the pivots cost six weeks of such labour!

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* Ast. Nach. No. 3046.

Monday, July 27, the actual anniversary, was marked by the performance of a singularly appropriate ceremony. Sir George turned on, for the first time, the gas which is to illuminate the Parish Church clock of Greenwich, and which will now be automatically and regularly turned on by the clock during the evening hours. The very hour (9 o'clock) at which he was thus once more concerned with Greenwich time was curiously in keeping with the occasion, and the excellent speech with which he concluded the ceremony bore evidence to his marvellous vigour. We sincerely hope to be present at an even more important celebration ten years hence.

THE DISC OF TITAN.—Herr Sarafik finds (Ast. Nach. No. 3046) for the disc of Titan, by comparison with the known diffraction discs of stars, a diameter of $0''.35$. His individual values range between $0''.14$ (in strong moonlight) and $0''.51$, and some correction for irradiation is therefore suggested. Prof. Pickering estimated Saturn as photometrically of the 0·9 magnitude, and Titan as 9·4; and thus, on the hypothesis of equal albedo, the diameter of Titan's disc would be $0''.33$. These accordant measures suggest a linear diameter of between 2000 and 3000 kilometres.

IT is with very deep regret that we hear of the death of His Eminence Cardinal Ludwig von Haynald, Archbishop of Kalocsa. It is now 12 years since he built the Observatory at Kalocsa, which has turned out some good work, more especially physical observations of the Sun.

MR. J. E. KEELER, of the Lick Observatory, has been appointed to succeed Prof. S. P. Langley at Allegheny.

THE Astronomische Gesellschaft will meet at Munich on the 5th of August.

PROF. E. GAUTIER of Geneva would be glad to receive observations of Comet 1890, III. (Coggia).

ERRATUM.—P. 265, line 21, *for* 1012 *read* 1006.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 179. SEPTEMBER. 1891.

The President's Address to the British Association.

"IT is now many years since this Association has done honour to the science of Astronomy in the selection of its President. Since Sir George Airy occupied the chair in 1851, and the late Lord Wrottesley nine years later in 1860, other sciences have been represented by the distinguished men who have presided over your meetings."

Dr. Huggins had thus a somewhat exceptional opportunity for describing the activity and progress of the last thirty years; and he used it with the ability which the world has recognized for nearly the period he reviewed. It is an ungrateful task to condense an address of which every sentence is interesting and important; but to reproduce the whole 34 pages *in extenso* is in many ways not feasible; and in apologetically offering the following brief notes to such of our readers as have not seen the original, we can only hope that they may be induced to obtain a copy of the 'Times' for August 20, or of the printed address from Messrs. Eyre and Spottiswoode.

We must congratulate Dr. Huggins on a signal success, which has been recognized equally by the general public and those more familiar with Astronomy. The President's portrait has been an attraction of our illustrated papers: he is "the Man of the Hour" in the most modern of them; and in one so ancient as to have celebrated its Jubilee there is a pictorial joke wherein a person who contrasts strongly with Dr. Huggins in all respects records his enthusiasm for the "Pres'dent Bri'sh Soshiashlem." On the other hand, those who were already familiar with Dr. Huggins and his work have learnt afresh almost to their surprise how closely he has been identified with the "very remarkable discoveries in our knowledge of the heavens which have taken place during this period of thirty years." Not that the President materially assists in pointing this moral: rather is it pointed by the facts in spite of him. He is almost too eager to assign credit to others where he might justly have mentioned his own work.

With one notable exception, all those who have in any way contributed to advance our knowledge find themselves, sometimes perhaps unexpectedly, gratefully named and recognized. The exception cannot be lightly passed over, and we cannot but feel that Dr. Huggins's friends will deeply regret his action in this matter. We must admit that he was placed in a very difficult position. Having been recently engaged in a scientific controversy where he felt keenly that he was not debating on simple facts, but about the vital principles of scientific method, he was apparently anxious on the present occasion to avoid saying anything which might be taken as a contribution to this discussion. But we cannot regard this as a sufficient reason for absolutely excluding the name of Mr. Norman Lockyer from a professedly historical account of the recent progress of Astronomy. Such an exclusion is so obvious as to precisely defeat its own object, by calling attention to the very topics made conspicuous by their absence.

But our province is rather summary than criticism; and we proceed to perform that difficult task with such misgivings as are natural to the uninspired. The more definite purpose of the address is declared to be "not to attempt a survey of the progress of spectroscopic astronomy from its birth at Heidelberg in 1859, but to point out what we do know at present, as distinguished from what we do not know, of a few only of its more important problems." Reference is first made to improvements in the spectroscope itself, the introduction of the concave grating, and matters relating to a standard scale.

"Lord Rayleigh has shown that there is not the necessary connection, sometimes supposed, between dispersion and resolving power, as besides the prism or grating other details of construction and of adjustment of a spectroscope must be taken into account.

"The resolving power of the prismatic spectroscope is proportional to the length of path in the dispersive medium. For the heavy flint glass used in Lord Rayleigh's experiments the thickness necessary to resolve the sodium lines came out 1·02 cm. If this be taken as a unit, the resolving power of a prism of similar glass will be in the neighbourhood of the sodium lines equal to the number of centimetres of its thickness. In other parts of the spectrum the resolving power will vary inversely as the third power of the wave-length, so that it will be eight times as great in the violet as in the red. The resolving power of a spectroscope is therefore proportional to the total thickness of the dispersive material in use, irrespective of the number, the angles, or the setting of the separate prisms into which, for the sake of convenience, it may be distributed.

"The resolving power of a grating depends upon the total number of lines on its surface, and the order of spectrum in use; about 1000 lines being necessary to resolve the sodium lines in the first spectrum.

"As it is often of importance in the record of observations to state the efficiency of the spectroscope with which they are made, Professor Schuster has proposed the use of a unit of purity as well as of resolving power, for the full resolving power of a spectroscope is realised in practice only when a sufficiently narrow slit is used. The unit of purity also is to stand for the separation of two lines differing by one-thousandth of their own wave-length; about the separation of the sodium pair at D.

"A further limitation may come in from the physiological fact that, as Lord Rayleigh has pointed out, the eye when its full aperture is used is not a perfect instrument. If we wish to realise the full resolving power of a spectroscope, therefore, the emergent beam must not be larger than about one-third of the opening of the pupil.

"Up to the present time the standard of reference for nearly all spectroscopic work continues to be Ångström's map of the solar spectrum, and his scale based upon his original determinations of absolute wave-length. It is well known, as was pointed out by Thalén in his work on the spectrum of iron in 1884, that Ångström's figures are slightly too small, in consequence of an error existing in a standard mètre used by him. The corrections for this have been introduced into the tables of the wave-lengths of terrestrial spectra collected and revised by a Committee of this Association from 1885 to 1887. Last year the Committee added a table of corrections to Rowland's scale.

"The inconvenience caused by a change of standard scale is, for a time at least, considerable; but there is little doubt that in the near future Rowland's photographic map of the solar spectrum, and his scale based on the determinations of absolute wave-length by Pierce and Bell, or the Potsdam scale based on original determinations by Müller and Kempf, which differs very slightly from it, will come to be exclusively adopted.

"The great accuracy of Rowland's photographic map is due chiefly to the introduction by him of concave gratings, and of a method for their use, by which the problem of the determination of relative wave-lengths is simplified to measures of coincidences of the lines in different spectra by a micrometer.

"The concave grating and its peculiar mounting, in which no lenses or telescope are needed, and in which all the spectra are in focus together, formed a new departure of great importance in the measurement of spectral lines. The valuable method of photographic sensitizers for different parts of the spectrum has enabled Professor Rowland to include in his map the whole visible solar spectrum, as well as the ultra-violet portion as far as it can get through our atmosphere. Some recent photographs of the solar spectrum, which include A, by Mr. George Higgs, are of great technical beauty.

"During the past year the results of three independent researches have appeared, in which the special object of the observers has been to distinguish the lines which are due to our atmosphere from those which are truly solar—the maps of M. Thollon, which, owing to his lamented death just before their final completion, have assumed the character of a memorial of him; maps by Dr. Becker, and sets of photographs of a high and a low sun by Mr. McClean."

We then pass on to consider how it gives us knowledge of the nature of the heavenly bodies. "The task of analysing the stars and nebulæ becomes one of very great difficulty when we have to do with spectra differing from the solar type... Under the ordinary conditions of terrestrial experiments the temperature or the mean *vis viva* of the molecules may have no direct relation to the total radiation. .. Very great caution is therefore called for when we attempt to reason by the aid of laboratory experiments to the temperature of the heavenly bodies from their radiation. . . . . No less caution is needed if we endeavour to argue from the broadening of lines and the coming in of a continuous spectrum as to the relative pressure of the gas in the celestial atmospheres. . . . . A continuous spectrum cannot be considered, when taken alone, as a sure indication of matter in the liquid or the solid state. . . . . Compound bodies are usually distinguished by a banded spectrum, but, on the other hand, such a spectrum does not necessarily show the presence of compounds... It may be thought that at least in the coincidences of bright lines we are on the solid ground of certainty; . . . . such would be the case if the coincidence were certain; but an absolute coincidence can only be a matter of greater or less probability: . . . . the progress of science has been greatly retarded by resting important conclusions upon the apparent coincidence of single lines, in spectroscopes of very small resolving power. . . . . The accuracy of the earlier determinations of the spectra of the terrestrial elements are in most cases insufficient for modern work on the Stars as well as on the Sun." It will be seen from these sentences that in this first section Dr. Huggins is chiefly anxious to point out what we do not know. He proceeds to concrete illustrations. "The spectroscope has failed as yet to interpret for us the remarkable spectrum of the Aurora Borealis, ... and especially we do not know the origin of the principal line in the green." . . . "In the spectra of comets the spectroscope has shown the presence of carbon presumably in combination with hydrogen, and also sometimes with nitrogen; and in the case of comets approaching very near the Sun, the lines of sodium, and other lines which have been supposed to belong to iron"; but "it would be perhaps unwise at present to attempt to define too precisely the exact condition of the matter which forms the nucleus of the comet. In any case the part of the light of the comet which is not reflected solar light can scarcely be attributed to a high temperature produced by the clashing of separate meteoric stones set up within the nucleus by the Sun's disturbing force. We must look rather to disruptive electric discharges produced probably by processes of evaporation due to increased solar heat, which would be amply sufficient to set free portions of the occluded gases into the vacuum of space. May it be that these discharges are assisted, and indeed possibly increased, by the recently discovered action of the ultra-violet part of the Sun's light? Lenard and Wolf have shown that ultra-violet light can produce a discharge from a negatively electrified piece of metal, while Hallwachs and Righi have shown further that ultra-violet light can even charge positively an unelectrified piece of metal. Similar actions on cometary matter, unscreened as it is by an absorptive atmosphere, at least of any noticeable extent, may well be powerful when a comet approaches the Sun, and help to explain an electrified condition of the evaporated matter which would possibly bring it under the Sun's repulsive action. We shall have to return to this point in speaking of the solar corona."

We come to more positive results in the case of the Sun. "A very great advance has been made in our knowledge of the constitution of the Sun by the recent work at the Johns Hopkins University by means of photography and concave gratings, in comparing the solar spectrum, under great resolving power, directly with the spectra of the terrestrial elements.... If the whole Earth were heated to the temperature of the Sun, its spectrum would resemble very closely the solar spectrum. . . . Prof. Rowland looks to the solar lines which are unaccounted for as a means of enabling him to discover new terrestrial elements. . . . He has already resolved yttrium spectroscopically into three components, and actually into two. . . . It is worthy of remark that as our knowledge of the spectrum of hydrogen in its complete form came to us from the stars, it is now from the Sun that Chemistry is probably about to be enriched by the discovery of new elements."

"In a discussion in the Bakerian Lecture for 1885 of what we knew up to that time of the Sun's corona, I was led to the conclusion that the corona is essentially a phenomenon similar in the cause of its formation to the tails of comets, namely, that it consists for the most part probably of matter going from the Sun under the action of a force, possibly electrical, which varies as the surface, and can therefore in the case of highly attenuated matter easily master the force of gravity even near the Sun. Though many of the coronal particles may return to the Sun, those which form the long rays or streamers do not return; they separate and soon become too diffused to be any longer visible, and may well go to furnish the matter of the zodiacal light, which otherwise has not received a satisfactory explanation. And further, if such a force exist at the Sun, the changes of terrestrial magnetism may be due to direct electric action, as the Earth moves through lines of inductive force. These conclusions appear to be in accordance broadly with the lines along which thought has been directed by the results of subsequent eclipses. Prof. Schuster . . . . asks whether the Sun may not act like a magnet in consequence of its revolution about its axis. Prof. Bigelow has recently treated the coronal forms by the theory of spherical harmonics, on the supposition that we see phenomena similar to those of free electricity. . . . Prof. Schaeberle, however, considers that the coronal phenomena may be satisfactorily accounted for on the supposition that the corona is formed of streams of matter ejected mainly from the spot-zones with great initial velocities. . . . . Of the physical and the chemical nature of the coronal matter we know very little. The continuous spectrum . . . . can only be due in small part to solar light scattered by small particles. . . . The green coronal line has no known representative in terrestrial substances, nor has Schuster been able to recognize any of our elements in the other lines of the corona."

Passing on to the stars, and the vexed question of the evolutional order as indicated by the spectra, Dr. Huggins mentions various views for the discussion of which "the few minutes which can be given to this part of the Address are insufficient." He therefore confines himself to stating briefly some of the considerations in favour of the evolutional order in which he arranged the stars from their photographic spectra in 1879. "This order is essentially the same as Vogel had previously proposed in his classification in 1874, in which the white stars represent the early adult and most persistent stage of stellar life, the solar condition that of full maturity and of commencing age; while in the orange and red stars with banded spectra we see the setting in and advance of old age;" and for this statement we must refer our readers to the Address itself.

The opening sentences of the next section we quote *verbatim* :—
"In the nebulæ, the elder Herschel saw portions of the fiery mist or 'shining fluid,' out of which the Heavens and the Earth had been slowly fashioned. For a time this view of the nebulæ gave place to that which regarded them as external galaxies, cosmical 'sand-heaps,' too remote to be resolved into separate stars; though, indeed, in 1858 Mr. Herbert Spencer showed that the observations of nebulæ up to that time were really in favour of an evolutional progress.

"In 1864 I brought the spectroscope to bear on them; the bright lines which flashed upon the eye showed the source of the light to be glowing gas, and so restored these bodies to what is probably their true place, as an early stage of sidereal life.

"At that time our knowledge of stellar spectra was small. For this reason partly, and probably also under the undue influence of theological opinions then widely prevalent, I unwisely wrote in my original paper in 1864, 'that in these objects we no longer have to do with a special modification of our own type of sun, but find ourselves in presence of objects possessing a distinct and peculiar plan of structure.' Two years later, however, in a lecture before this Association, I took a truer position. 'Our views of the universe,' I said, 'are undergoing important changes; let us wait for more facts with minds unfettered by any dogmatic theory, and therefore free to receive the teaching, whatever it may be, of new observations.' "

Reference is then made to the light thrown upon the nebular hypothesis in recent years: the work of Prof. George Darwin, and "some thinkers" on meteorites; Mr. Roberts's photograph of the nebula in Andromeda; and, finally, spectroscopic work on the nebulæ, a subject so peculiarly Dr. Huggins's own, that we must reverently accept his dictum that no certain conclusions have yet been reached as to the origin of the nebular lines. The question is suggested as to whether we cannot go even one step backward from nebulæ, and trace their origin in the collision of dark suns. The work of Darwin aad Poincaré on rotating masses of fluid, and the remark of Mr. See that the solar system is quite an exceptional formation in the relatively large size of the parent body, are alluded to.

The Address then passes to the use of the spectroscope in metrical astronomy, for determining motions of stars in the line of sight. Here, again, the observational difficulties are so well known to Dr. Huggins, who originated the research in 1868, that his satisfaction with the apparently complete success of Dr. Vogel and Mr. Keeler must be considered of great significance, though a sudden diminution of the probable error of a class of observations to about one-tenth of its previous value, by improvements of no overwhelming importance in instruments, is at least unprecedented in the history of Astronomy. "My instrumental means, limiting my power of detection to motions greater than 25 miles per second, were insufficient. Mr. Keeler has found in the examination of ten nebulæ motions varying from two to twenty-seven miles, with one exceptional motion of nearly forty miles.

"The spectroscopic method of determining celestial motions in the line of sight has recently become fruitful in a new but not altogether unforeseen direction, for it has, so to speak, given us a separating power far beyond that of any telescope... The spectroscope has not simply added to the list of the known binary stars, but has given to us for the first time a knowledge of a new class of stellar systems, in which the components are in some cases of nearly equal magnitude and in close proximity, and are revolving with velocities greatly exceeding the planetary velocities of our system...... The telescope could never have revealed to us double stars of this order. In the case of β Aurigæ, an object-glass of about eighty feet in diameter would be needed to resolve this binary star."

Dr. Huggins then passes to the "new power which modern photography has put into the hands of the astronomer. . . Photography is no new discovery, being about half a century old. . . . The recent successes in astronomical photography are not due to greater skill, nor, to any great extent, to superior instruments, but to the very great advantages which the modern gelatine dry plate possesses for use in the observatory over the methods of Daguerre, and even over the wet collodion film on glass. . . Two principal directions may be pointed out in which photography is of great service to the astronomer. It enables him within the comparatively short time of a single exposure to secure permanently, with great exactness, the relative positions of hundreds or even thousands of stars; or the minute features of nebulæ or other objects, or the phenomena of a passing eclipse." And, secondly, "by recording light-waves which are both too small and too large to excite vision in the eye, it brings him into a new region of knowledge, such as the infra-red and the infra-violet parts of the spectrum, which must have remained for ever unknown but for artificial help."

There follow a brief account of the proposed "Photographic Chart and Catalogue of the Heavens," references to Dr. Gill and Prof. Kapteyn's Southern Durchmusterung; to questions affecting the photographic brightness of stars; to Prof. Pritchard's photographic method of determining parallax; to the nebular and stellar photographs of Dr. Common and Mr. Roberts; and to Mr. Hale's photographs of the spectra of the solar prominences.

Passing to the configuration of the stars, Dr. Huggins says:—"We see a system of systems, for the broad features of clusters and streams and spiral windings which mark the general design are reproduced in every part. . . The deciphering of this wonderfully intricate constitution of the heavens will be undoubtedly one of the chief astronomical works of the coming century." And then, after mentioning "some of the problems of the newer side of astronomy," to which time does not permit a fuller reference, he concludes:—"Astronomy, the oldest of the sciences, has more than renewed her youth. At no time in the past has she been so bright with unbounded aspirations and hopes. Never were her temples so numerous, nor the crowd of her votaries so great. The British Astronomical Association, formed within the year, numbers already about 600 members. Happy is the lot of those who are still on the eastern side of life's meridian!

"Already, alas! the original founders of the newer methods are falling out—Kirchhoff, Ångström, D'Arrest, Secchi, Draper, Becquerel; but their places are more than filled; the pace of the race is gaining, but the goal is not, and never will be, in sight.

"Since the time of Newton our knowledge of the phenomena of Nature has wonderfully increased; but man asks, perhaps more earnestly now than in his days, what is the ultimate reality behind the reality of the perceptions? Are they only the pebbles of the beach with which we have been playing? Does not the ocean of ultimate reality and truth lie beyond?"

New Nebulæ in Cygnus.

Celestial photography has inured us to surprises. In attempting to estimate its future performances we instinctively expect the unexpected and count upon the incalculable. New nebulæ, above all, have by its means been brought to notice in such manifold guises, and under circumstances so singular, as well-nigh to exhaust the stock of available public wonder. Thus, to Dr. Max Wolf's latest announcement many preludes had been struck; in some respects it might be considered a mere sequel to what had gone before—a reiteration and confirmation of important truths already ascertained rather than an essential novelty. Otherwise looked at, however, it appears a beginning, a promise, and, even as it stands, a disclosure of no small moment.

During the last year and half, Dr. Wolf has been engaged at Heidelberg in photographically exploring faintly nebulous regions of the sky. The portrait-lenses employed by him for the purpose afford, because of their short focal lengths, small but intensely bright images, and are hence, as is now well known, particularly adapted for constraining diffused light, too feeble for the production of any effect in ordinary astronomical instruments, to become sensible. With one of these and an exposure of 5½ hours, he obtained, on January 2 last, a remarkable picture of the nebulæ surrounding ζ Orionis, earlier however, apparent on Professor Pickering's plates; and his investigations in several parts of the constellation Monoceros have been effective both for discovery and for assuring and extending the discoveries of others. But these results were only preliminary to one of, it would seem, higher significance. Vaguely discernible, a little more than one degree to the south-east of α Cygni, an ill-defined nebulosity (Gen. Cat. 4621) was observed and catalogued by Sir John Herschel. This now proves to be only the most conspicuous part of a vaster formation. Three Heidelberg photographs—the last taken June 1, 1891, in three hours, with a 5-inch Kranz aplanatic—show a large nebula, of excessively fine and complex texture, spreading outward from a central point located approximately in R.A. $20^h\ 54^m$, Dec. +44°. Thence to the east and north, branches issue to a distance of at least eight degrees, meeting and involving dense galactic star-groups: a westerly extension passes between 55 and 57, and reaches α Cygni; a distinct ramification condenses near 56 and 57 Cygni; while from α Cygni to the north, by 51, ω^1, ω^2, and ω^3 Cygni, and as far to the west and south, fold upon fold of nebulous matter surges over the sky, becoming notably compact and luminous in the immediate neighbourhood of γ Cygni. Dr. Wolf finds in these records unequivocal evidence that the bright stars in the region covered by them are in genuine nebulous connection with each other and with the Milky Way accumulations they emblazon [*]; and his description of them certainly seems to leave room for no other inference.

The importance of such an inference, it is easy to see, is fundamental; for we cannot stop short at barely admitting without further generalizing it. Its proved validity would then oblige us to regard the whole "golden Galaxy" as one prodigious mixed system, resembling that of the Pleiades in point of composition, though very different from it in plan of structure. Visibly nebulous in parts, it would seem to possess everywhere nebulous affinities; and the progress of research tends more and more to exhibit it in the character of a teeming hive of cosmical bodies, not yet wholly disengaged from their primitive chaotic surroundings. The study of nebular distribution might alone, and long ago, have driven out of the field every form of "projection theory" of the Milky Way. For it showed the great majority of gaseous objects to be embraced within its circuit, and this alone amounted to a demonstration that a physical reality, and not simply a geometrical appearance, was in question. The almost exclusive occurrence within the same zone of temporary and bright-line stars confirms the teaching of the nebulæ, which is still further enforced by the predominance there of stars showing Sirian spectra. This remarkable feature, disclosed by the concurrent indications of Dr. Gill's and Prof. Pickering's photographs, extends to the faintest stars of the Milky Way so far photometrically determined; and the deduction is a legitimate one that the glimmering star-dust aggregated into galactic clouds shines too with the Sirian quality of light.

That these clouds, in some cases at any rate, own relations of mutual dependence with lucid stars, seems placed beyond doubt by the Heidelberg photographs [†]. The brilliant orbs shown in them to be intertwined, by means of sinuous wreaths of nebula, with minute clustering objects, must plainly belong to the same scheme of generative activity. All then co-exist in one great realm of space, at virtually equal distances from ourselves. We are, however, completely ignorant as to the scale of those distances. Dr. Elkin has indeed measured α Cygni for parallax, but with only negative result. This, however, does not necessarily signify that the star really lies beyond the range of such determinations. It may mean nothing more than what is now, on other grounds, rendered highly probable—namely, that the measures were, in their upshot, vitiated by the systematic connection of α Cygni with the neighbouring stars chosen for purposes of comparison. The question, in other words, is still open as to whether the star has or has not a sensible *absolute* parallax.

Indeed, experiments for relative parallax with stars undistinguished by proper motion from the test-objects in their vicinity can scarcely be expected to lead to satisfactory results. Disparity in brightness, taken by itself, is very far from proving separation by wide spatial intervals; and the association of large with small stars into immense physical groupings has of late been so frequently recognized, that their disjunction in any particular case cannot safely be assumed, and can only be established by observed disparities of movement. Parallactic operations of the relative kind upon immobile stars may then easily prove nugatory, and they were presumably so as regards α Cygni.

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[*] Astron. Nach. No. 3048.

[†] Mr. Ranyard has recently collected some important evidence to this effect.

Neither α nor γ Cygni possesses any reliably ascertained proper motion, for the shift northward over an arc of about 2″ a century, nominally attributable to the latter, need not count as such. This negative conformity, taken by itself, affords indeed no presumption of physical connection; but significance is imparted to it by the precise agreement of the two stars in the direction and amount of their radial movement. If this be a coincidence, it is a sufficiently remarkable one, and it will be of the greatest interest to determine whether any other of the nebulously involved stars in Cygnus share this drift of six kilometres a second in our direction. It may be, indeed, that the drift is our's. The velocity of the solar system is, at the lowest estimate, fully adequate to account for the advance towards it ascribed at Potsdam to these two bright objects, which, for anything that appears to the contrary, may, with their stellar and nebular appurtenances, be moored unchangingly " In the vast ocean of unbounded space."

A. M. Clerke.

Periodic Comets.

Wolf's comet is now pretty conspicuous and easily visible in a small telescope. The following ephemeris by Berberich indicates its path:—

	R.A.	Dec.	Light.
	h m s	° ′	
Sept. 8	3 49 1	+22 57	8·64
16	4 4 34	20 15	9·59
24	4 17 45	17 1	10·48
Oct. 2	4 28 13	13 18	11·27
10	4 35 41	9 12	11·84
18	4 40 6	4 53	12·10
26	4 41 33	+ 0 31	11·98
Nov. 3	4 40 19	− 3 37	11·44
11	4 36 56	− 7 17	10·55

On Sept. 4 the comet will be placed on the N. borders of the Pleiades. It passes through the Hyades on Sept. 23–28, and then moves southwards about $\frac{1}{2}$° daily. On Oct. 27 the comet crosses the equator, and soon after enters Eridanus. Its greatest intensity of light will be attained between about Oct. 18–22.

Encke's comet will be close to θ Geminorum on Sept. 2–3, and is pursuing a rapid course eastwards. On Sept. 8 its position is 2° N. of α Geminorum. Backlund has computed the following ephemeris:—

	R.A.	Dec.
	h m s	° ′
Sept. 5......	7 2 24	+34 44
9......	7 35 36	33 41
13......	8 10 25	31 58
17......	8 45 49	29 30
21......	9 20 59	26 17
25......	9 55 0	22 26
29......	10 27 27	+18 5

Prof. Barnard observed the comet on August 1, and found the correction (O – C) to above ephemeris

[See p. 320.] $\Delta\alpha = -16^s$, $\Delta\delta = -3'{\cdot}7$.

The comet passes quickly through Gemini and Cancer, and on Sept. 27 will be found very near γ Leonis.

Tempel-Swift's comet is expected at perihelion in November next, and M. Bossert has published an ephemeris, from which we extract a few positions for the next few months.—

	R.A.	Dec.	Light.
	h m s	° ′	
Sept. 4......	21 33 2	– 2 18	2·66
Oct. 2......	21 6 24	+ 2 30	6·30
Nov. 3......	21 44 11	13 2	14·09
27......	23 43 49	26 42	19·68
Dec. 29......	3 34 45	+29 36	6·94

The comet is getting rapidly brighter, and is at present situated in the N.W. region of Aquarius. At the beginning of December it will be close to α Andromedæ.

W. F. D.

Selenographical Notes.

HAINZEL.—This remarkable object affords one of the most instructive examples of a not uncommon phenomenon on the moon's southern hemisphere, the intrusion of one ring-mountain on another. Neison describes it as "a pear-shaped walled-plain 55 miles in diameter," but this description does not truly represent its actual structure. It consists, in fact, of a very large and lofty ring which would have been circular, or nearly so, had not a second ring of somewhat smaller dimensions subsequently intruded on the northern side of it, and thus modified the appearance of what would otherwise be a formation of the normal type.

Mädler's drawing of Hainzel resembles the imprint of a human foot on soft ground—the intrusive ring on the north being in part represented by the heel, and the larger ring by the sole; but there is nothing to indicate the effects of the interference of two distinct formations,—on the contrary, by the ridges on the floor, which are shown as uninterruptedly traversing the object from south to north, and by the nearly complete continuity of the walls, his map would lead one to conclude that it is nothing more than a single ring-plain of a somewhat irregular shape. Schmidt presents a true picture of what anyone with suitable optical means may observe in connection with Hainzel when the E. long. of the morning terminator is between 42° and 55°, viz., a prominent and nearly perfect circular ring whose centre lies near the place where the northern border of a somewhat larger ring on the south of it would be were it complete. The massive walls of these formations are represented as independent of each other, or, more accurately, as showing no traces of continuity—the larger ring embracing and including a portion of the smaller.

Between 10^h and 11^h on April 19, this year, the interior was only a little more than half illuminated,—the shadow of the lofty eastern flanks of the associated formations obscuring much of the curious detail visible at a later stage on the western side of the floor of the larger or more southerly ring, but the details on the opposite border and on the eastern section of both interiors were displayed with admirable distinctness with a power of 350. First, as regards the inner slope of the broad eastern rampart of the larger ring:—a terrace on this declivity just below the crown of the wall forms with the summit ridge one of those longitudinal chasms, crossed at intervals by dykes or rock-partitions, which are so frequently noted in connection with large formations and which seem often to be due to rows of inosculating depressions. This valley is terminated at each end by a prominent crater. Two or three narrow spurs or buttresses run down the slope at right angles to this terrace (against which they abut), in every respect similar to those observed outside this and many other great rings. They were not so obvious as at an earlier phase, but dark streaks, representing the intervals between them, were prominent on the bright surface of the declivity. There is also a well-marked terrace on the inner east wall of the smaller ring which, together with a bright crater at its foot (probably Neison's *n*), was well seen. At this stage the existence of a southern section to the intruded formation is plainly indicated by the continuation of the bright eastern wall partially across the interior of the combined walled-plains and, where this wall apparently dies out, by small isolated mounds on the floor.

On the following night, April 20, when the terminator had advanced to about east long. 54°, and the shadow of the eastern walls no longer obscured objects within the enclosure, the individuality of the northern portion of Hainzel was very manifest. It appeared as an almost complete light ring overlapping the larger and more irregular formation with which it is associated,—in fact much as it is shown by Schmidt in his excellent representation of it. Under a solar altitude of 10° or thereabouts a very conspicuous wide ridge is observed to run for some distance in a south to a north direction along the centre of the larger inclosure and, bending towards the south-west, to terminate at the more easterly of two large craters near the summit of the south-west wall. It bifurcates, however, at the bend—the branch running up to the more northerly of two still larger craters which occupy a gap in the southern border. At this stage of sunrise the curved portion of the ridge stands out brightly amid the black shadow of the wall. At its northern end, just outside the intrusive ring, it turns suddenly to the west, and it is noteworthy that the curvature of this bend is concentric with that of the low southern wall of the ring. An interval of 24 hours reveals the true character of these features. The longitudinal ridge with its bend to the north-west wall, and the bend just referred to, form a lofty partition, dividing the southern part of Hainzel into two distinct sections. The more easterly of these divisions is the larger, and, as sunrise advances, the dark tone of its interior contrasts strongly with the lighter hue of the floors of the western division and of the northern intrusive ring. It is impossible to speak with certainty as to the relative heights of the interiors of the two inclosures included by the southern section of Hainzel and of the floor of the intrusive ring, but from all appearance there is little or no difference between them. The western rampart of the formation is of a very uniform altitude, being, according to Neison, generally about 11,500 above the interior, while it is only some 1000 feet less above the surrounding country. There are indications of what seems to be an old crater-ring of considerable size with dark interior adjoining the west side of the northern section of Hainzel. Though not shown on the maps, it is traceable for some time after sunrise, and was even visible at 9^h on April 20, when the morning terminator had advanced to E. long. 54°. Schmidt shows a rill extending from near Hainzel B (one of a row of large craters north and north-west of Hainzel) to the north-west border, and then, maintaining the same direction, across the floor of the northern ring up to the inner foot of its eastern wall. The exterior portion of this cleft was well seen on April 19, but could not be traced on the floor, which appeared to be free from detail, though Mädler represents it covered with ridges. Two or three long narrow light streaks were noted close to this cleft and running parallel to it. They are probably shallow longitudinal valleys and not true rills.

Beaumont House, Shakespeare Road,
Bedford, 1891, Aug. 19.

THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Bliss, Astronomer Royal.

GENTLEMEN,—

In Vol. ix. of the 'Observatory' I communicated a few particulars respecting Prof. Bliss, who succeeded Bradley as Astronomer Royal in 1762, and had acted from time to time as his deputy before that.

I have since been somewhat surprised to read, in the account of Bliss in the 'Dictionary of National Biography,' the following remark in speaking of his observations:—

"Although including only what was indispensable in order to deduce the places of the Sun, Moon, and planets at the most important points of their orbits, they are of value as being made on Bradley's system and with Bradley's instruments; yet they have never been reduced."

In the stupendous work effected by Sir George Airy in the earlier part of his tenure of office as Astronomer Royal, that of reducing the lunar and planetary observations of his predecessors from the year 1750 to 1830, those were included which had been made by Bliss as well as by the assistant Green, who continued them during the months which intervened between the death of Bliss in 1764 and the appointment of Maskelyne in 1765.

I may here take the opportunity of remarking that although the Lee parish-registers, quoted both in Lysons's 'Environs of London' and Hasted's 'History of Kent,' show that Bliss was buried there on September 4, 1764, two days after his death at the Royal Observatory on September 2, it does not appear that any stone was placed as a memorial; at any rate, if one ever was, the inscription on it (as I can state after several searches) has become illegible. In Sir George Airy's Report for 1855, he speaks of the restoration of the tomb in which Halley and Pond were buried; and says that the stone in memory of Halley (now at the Royal Observatory) was "so much injured that mere repair appeared hopeless." A rubbing of the inscription was therefore taken, and the same cut upon a new stone, the new inscription being a rigorous copy of the old one. The former is now considerably worn (owing to the influence of the weather in its horizontal position), and the final Æ looks at first sight like A.

Yours faithfully,

Blackheath, 1891, Aug. 5. W. T. LYNN.

Ancient Eclipses.

GENTLEMEN,—

Mr. Pinches communicated some time ago to the 'Babylonian and Oriental Record' transcriptions and translations of cuneiform inscriptions on a Babylonian tablet in the seventh year of Cambyses, king of Persia, which record two eclipses of the Moon on the 14th of the months Tammuz and Tebet respectively in that year. Mr. Pinches did not identify the eclipses, but I do not think we need hesitate to regard them as those which are tabulated in the 'L'Art de vérifier les Dates,' as having occurred on the 5th of July and the 30th of December respectively in the year B.C. 522, thus confirming the date of the accession of Cambyses. The Babylonian months being lunar, the eclipses would occur on the same day of each month.

Mr. Stockwell has communicated another article to the 'Astronomical Journal' (No. 244), in which he contests some of my remarks in the June number of the 'Observatory.' I have written to the 'Astronomical Journal,' and need only ask of you space for a very few lines on the subject. Mr. Stockwell has rightly noticed a *lapsus plumæ* on my part in line 25 of p. 235 of the 'Observatory,' where "before" should be "after"; but this does not affect the conclusion. I must acknowledge another inadvertence in p. 234, line 9, where "preceded" should be "followed." That the Olympic festivals followed by one year the years which corresponded to bissextile years in the Julian calendar is (as I remarked before) not a matter of opinion, but has been sufficiently proved in 'L'Art de vérifier les Dates,' vol. i. pp. xxiii & xxxix.

Mr. Stockwell quotes Liddell as his authority for saying that in the year B.C. 81 (undoubtedly corresponding to a bissextile) an Olympic festival was suspended by Sulla on account of the spectacles he was exhibiting after his triumph for his successes in the Mithradatic War. Appian is the source of this statement, but it is not clear from his language how long after the triumph (held at the beginning of February) the festival was to have taken place which was in great part suspended. Sulla was dictator all through B.C. 80 (some think the triumph was in that year), and many of the spectacles may have been exhibited in that year.

Mr. Stockwell cites Josephus in saying that Augustus Cæsar was 77 years of age at the time of his death, and by taking A.D. 13 for the date of this, he carries back his birth to B.C. 65, corresponding to a bissextile. The same year Jerusalem was taken by Pompey, and Josephus says that this was in the 179th Olympiad. But he does not say (as Mr. Stockwell quotes him) that it was in the *first year* of that Olympiad. Few dates in ancient Roman history are better established than that of the birth of Augustus and the capture of Jerusalem by Pompey. It was the same year in which Cicero thundered forth against Catiline, and there is scarcely any doubt that it was B.C. 63. Now Suetonius (surely a preferable authority to Josephus on such a point) says that Augustus was in his *seventy-sixth* year at the time of his death. From B.C. 63 ($=-62$) to A.D. 14 would be 76 years, and as Augustus in A.D. 14 would almost have completed his

76th year, I contend that this fully confirms that year as the date of his death.

Mr. Stockwell is somewhat merry over his implication, that I thought Julius Cæsar needed the light of the Moon to add to the light of the Sun at midday. But I meant that the historian mentioned the visibility of the Moon at midday only as a remarkable circumstance. I gave not a translation like Mr. Stockwell, but the original passage, and still think that "hora VI," without any qualifications, means the sixth hour of the *day*. It will be noticed that the word "waited" in the translation does not occur in the original.

Yours faithfully,

Blackheath, 1891, Aug. 15. W. T. LYNN.

The Chinese Eclipse of B.C. 776.

GENTLEMEN,—

Some time ago I alluded to an eclipse of the Sun which occurred on September 6, B.C. 776, and is mentioned in a Chinese work edited by Confucius (Kung Kew) or one of his disciples about two centuries afterwards. As this is probably the most ancient record existing of an eclipse which can be identified, it seemed to me that a reference to the original might interest some of your readers. But I had much trouble to find it because there is a mistake in 'L'Art de vérifier les Dates,' where it is stated to be in Tchun-tsieou, whereas it is really in a collection of poems called the 'She-king.' Father Gaubil, however, gives the correct reference in the second volume of the 'Observations,' edited by Souciet at Paris in 1729–32, and also points out that the eclipse occurred in the sixth year of the reign of the emperor Yeou-wang, corresponding to the first year of the first Olympiad and to the year B.C. 776 of our chronology. The record is in the ninth ode of the fourth book of the 'She-king,' the former part of which, as translated by Dr. Legge in the fourth volume of his 'Chinese Classics,' runs thus:—

1.

"At the conjunction [of the Sun and Moon] in the tenth month
On the first day of the Moon, which was *sin-maou*,
The Sun was eclipsed,
A thing of very evil omen,
Then the Moon became small,
And now the Sun became small,
Henceforth the lower people
Will be in a very deplorable case.

2.

"The Sun and Moon announce evil
Not keeping to their proper paths,
All through the kingdom there is no [proper] government,
Because the good are not employed.
For the Moon to be eclipsed
Is but an ordinary matter;
Now that the Sun has been eclipsed,
How bad it is!"

I should mention that Dr. Legge in his note on the place erroneously gives the date as Aug. 29, B.C. 775; the eclipse really took place in B.C. 776 (= A.D. −775) on Sept. 6.

Yours faithfully,
W. T. LYNN.

Blackheath, 1891, June 27.

[The above letter was intended to appear in our August number *instead of* the last two paragraphs (occupying 16 lines, on p. 286) of Mr. Lynn's letter on "Assyrian Eclipses," dated June 12; the proofs, however, were unfortunately mislaid, and we must therefore ask our readers to erase those paragraphs in their copies, and also to read B.C. 669 for B.C. 699 in line 2 of the same page.—EDS.]

Transit of Mercury.

GENTLEMEN,—

From this month's 'Observatory' I find that during the recent Transit of Mercury neither the late Mr. Pogson nor Prof. Michie Smith at Madras observed the usual "halo" round the planet's disc. I think, therefore, that the following extract from a paper that I have submitted to the R. A. S., embodying my observations of the transit, may prove interesting:—

"The aureola was best seen when the darkest part of the Pritchard wedge was interposed, and was gradually lost sight of as the wedge was shifted and its lighter portions brought opposite the eye, even when the depth of colour of the wedge was sufficient to enable the general surface of the Sun to be observed not only with complete safety but also with comfort."

From the repeated observations made by me of the halo during the transit, I feel confident that the Madras observers would not have failed to notice it if they had either changed the "dark glasses" or employed a wedge.

Yours faithfully,
K. D. NAEGAMVALA.

The Observatory, College of Science,
Poona, 1891, July 29.

Observing Occultations.—Misuse of Scientific Symbols.

GENTLEMEN,—

The method which I described to Mr. Innes as useful for observing occultations, and which it appears has been employed for many years by Capt. Noble, is of course a good one when the star both at the disappearance and reappearance is at a considerable altitude. At low altitudes the method fails in consequence of the unequal refraction for the two places of the occultation. Under the same condition Mr. Tatlock's improved method will of course likewise fail.

Whilst I am a lover of perspicuity in scientific descriptions, I may say that I am also a cordial hater of anything in the shape of ambiguity. I therefore heartily sympathize with Capt. Noble in his protest against the use of the signs ′ ″ for feet and inches or for minutes and seconds of time. I have on two or three occasions made the same protest here, notably in the case of a civil engineer who published certain data of mine expressed in feet and inches as if they were minutes and seconds of arc. Lamont's well-known catalogues of stars are disfigured by the misuse of ′ ″ for minutes and seconds of time.

While on this subject I may state that I have also on several occasions protested against the method almost invariably adopted by the correspondents of our leading daily journal for expressing the amount of rainfall. It is almost always given in "points." It appears that some years ago the present Government Astronomer found that his observing staff generally throughout the Colony had so little idea of the nature of decimals that their reports of the rainfall came to him in a very ambiguous state. Accordingly, in his printed forms he requested the observers to state the quantity of rain not in decimals but in "points" or "divisions" of their gauges. This was, of course, intended only as a private arrangement between the head of the meteorological department and the observers in order to avoid ambiguity in the returns. In his own official publications he has always translated the "points" into hundredths of an inch. The term "points" has now, however, come into public use, and those who favour it are apparently determined that it shall maintain its ground. I have been frequently interrogated as to the meaning of the term, but have always replied that it is not recognized in scientific circles, but is merely employed by persons unacquainted with the simple nature of decimals to express hundredths of an inch. Yours faithfully,

Windsor, N. S. Wales, 1891, July 12. JOHN TEBBUTT.

[The use of ′ and ″ for minutes and seconds of time is of course unpardonable, though we do not quite see the enormity of using them for feet and inches, where no confusion is likely to arise. It disturbs us, however, to see such signs placed after a long decimal, as 0·3843″ instead of 0″·3843.—EDS.]

Total Solar Eclipse, April 16, 1893.

MESSIEURS,—

L'éclipse totale du soleil, qui doit avoir lieu le 16 avril, 1893, se produisant dans une région sur laquelle nous possédons peu de données au point de vue de la nébulosité du ciel, j'ai prié le directeur du chemin de fer du Céara de faire faire pendant le mois d'avril de cette année des observations météorologiques à 9^h du matin, midi et 3^h soir. Comme vous pourrez le vérifier par un coup d'œil jeté sur notre revue du mois de mai dernier*, nous y avons publié le détail de ces observations, et à la page 67 j'ai résumé les données concernant la pluie et la nébulosité. Il résulte de ces observations qu'il y a eu 20 jours de pluie, ayant fourni

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* 'Revista do Observatorio,' anno vi. maio de 1891, no. 5.

une hauteur de 262 $^{m}/_{m}$, et que la moyenne de la nébulosité a été de 6,5. Ces observations ont été faites à Fortaleza, capitale du Céara, désignée sur beaucoup de cartes sous le nom de Céara; ce point est compris dans la zône de la totalité et à environ 15 à 20 kilom. S.E. de la ligne centrale (intersection avec le littoral).

Ces observations seront reprises au mois d'avril 1892, de manière à réunir des données qui permettent de se former une opinion assez sûre au sujet des probabilités du temps pour l'année 1893.

Agréez, je vous prie, Messieurs, l'assurance de mes sentiments les plus distingués.

Observatorio do Rio de Janeiro,
25 juillet, 1891.

L. Cruls, *Directeur.*

Jupiter.

Gentlemen,—

On Aug. 21, 1891, some interesting details were presented on the disk of this planet.

A large white spot was situated on the great S. equatorial belt, and it crossed the central meridian at 12^{h} 10^{m}. Running to the N.N.E. from this marking and crossing the equator, where it joined the great N. equatorial belt, there was a stream of dark material closely following another marking of similar character.

On the double belt N. of the great N. equatorial belt (also double) I saw a very conspicuous black spot, quite as obvious as the shadow of a satellite, but perhaps a little smaller. This object became central at 12^{h} 41^{m}. In 1880 there was a remarkable outbreak of black spots on this belt, and they moved with a velocity greater than that of any other Jovian features, for the period of rotation, as I determined it, was 9^{h} 48^{m}. It will be important to watch the spot now visible, as it possibly augurs a renewal of the phenomena of 1880.

The red spot was central at 13^{h} 2^{m}. It was very distinct, and appears to be recovering its brick-red colour. Any small telescope will now show the spot, but it is not such a striking feature as it was in 1879–81. There is at present a very brilliant spot close to its S. margin and opposite its centre, and this looks very conspicuous from contrast with the red spot and dusky belts outlying it.

I used a 10-inch reflector, power 252.

Yours faithfully,
W. F. Denning.

Bristol, 1891, Aug. 23.

Jupiter's Satellite-shadows.

Gentlemen,—

On Friday, August 14, 1891, I observed the apparently rare phenomena of the overtaking and overlapping of the shadow of Sat. II. by the shadow of Sat. I. on the disc of Jupiter. It was evident, on inspection of the 'Nautical Almanac,' that this might occur, for Sh. II. was to enter ♃'s disc before Sh. I. and to

leave it after. From the data given in the almanac I anticipated a conjunction of the shadows at about $11^h\ 30^m\ 44^s$, G.M.T. The telescope I employed was a 6-inch refractor by Simms, focus 7 feet, power 180. The observations made were as follows:—

		h m s	h m	
II. Sh. Ingr., bisection of limb,	G.M.T.	10 56 44	10 51	By the 'Nautical Almanac.'
I. Sh. Ingr., „	„	11 3 38	10 59	
I. Sh. on central meridian of ♃,	„	12 7 36	..	
II. Sh. „ „	„	12 15 41	..	
I. Sh. Egr., bisection of limb,	„	13 14 33	13 18	
II. Sh. Egr., „	„	13 41 30	13 45	

The conjunction of shadows might be inferred from the observed ingresses and egresses at $11^h\ 42^m\ 33^s$ G.M.T., but I have the following observing notes as made at the time:—

At $11^h\ 26^m\ 51^s$ the overlapping shadows of I. and II. have now coalesced, and are very black upon the southern equatorial dark belt and partly north of it. At $11^h\ 39^m\ 35^s$ the joint shadow *seems quite round and very black*; it has overtaken the red spot by one third of that spot; the view very good. At $11^h\ 44^m\ 36^s$ the broadening of the south equatorial belt following the red spot is very manifest; the preceding end of the red spot is on the central meridian; the joint shadow of I. and II. *still quite round.* At $11^h\ 52^m\ 6^s$ the longer axis of the overlapping shadows is inclined 45° to ♃'s equator; the centre of the joint shadow has nearly overtaken the middle of the red spot. At $12^h\ 19^m\ 51^s$ the red spot is fully centred on ♃, perhaps a little past (by Marth's ephemeris $12^h\ 14^m\ 42^s$).

If we take the mean of $11^h\ 39^m\ 35^s$ and $11^h\ 44^m\ 36^s$, we have a resulting time $11^h\ 42^m\ 5^s{\cdot}5$ G.M.T. as that at which the two shadows were seen in overlapping conjunction. The shadow of II. was always just wholly within the south equatorial dark belt, and the centre of the shadow of I. seemed always on the north edge of this belt.

The same evening I had observed an overlapping conjunction of Sats. I and II. east of ♃, at $9^h\ 50^m\ 51^s$ G.M.T. For 90 seconds before and after this time the satellites formed one mass, nearly round, as large as Sat. III., and rather brighter. Both were going towards the disc of ♃. They overlapped to the extent of a radius of either satellite.

Yours faithfully,

A. Freeman.

Murston Rectory, Sittingbourne,
1891, Aug. 20.

Merle's Comet.

Gentlemen,—

Some years ago, on looking over an old volume of the 'Philosophical Transactions' for 1685, I noticed the following remarks of one Dr. Plot on p. 931:—"W. Merle, Fellow of Merton College, observed the weather at Oxford every day of the month seven years together, viz. from January 1337 to January 1344, the MS. copy of which observations are yet remaining in the Bodleyan Library." It seems this work, through the exertions of G. J. Symons, F.R.S., the meteorologist, has just been photographed and reproduced with translation. Although it is more interesting to the meteorologist than to the astronomer, there is an entry under the date of September 1343 worth bringing forward. "In the second week a comet (ardens draco) was seen about sunset, or before, in Lyndesay, which betokened future dryness then." Lyndesay is a portion of Lincolnshire. In a 'Prospect of the most famous parts of the World,' 1646, it is stated that the division of Lincolnshire is into three principal parts, viz., Lindsey, Kesteven, and Holland, and that Lindsey is subdivided into seventeen hundreds.

Now, in Chambers's Catalogue no comet is mentioned from A.D. 1340 to 1345; in Williams's 'Chinese Comets' none from 1340 to 1351. Consequently we have here, as it seems, the sight of a hitherto unrecorded comet mentioned by an eye-witness. It is greatly to be hoped that the other portion of Merle's works on the eclipses of 1345, 1349, 1363, and on several conjunctions of Jupiter and Saturn and of Mars and Saturn in that century may be reproduced in like manner.

Faithfully yours,

Melplash Vicarage, Bridport,
1891, Aug. 27.

S. J. JOHNSON.

Berne Time and the 'Nautical Almanac.'

GENTLEMEN,—

There is something so Arcadian in the simplicity of Mr. Backhouse as to be positively touching. Can he have considered, for an instant, that the insertion of the geographical co-ordinates of the Berne Observatory in the 'Nautical Almanac' would involve a slight amount of *trouble*? or reflected how very much easier it is to go on, year after year, grinding out the same details from the same tables (as the "Bogie Man" is ground out on a street organ) than to introduce any fresh matter whatever for the use and convenience of Astronomers? Really, to read his letter, one would think that Dr. Hind existed for the Office of Superintendent of the 'Nautical Almanac,' instead of that Office existing for him!

Faithfully yours,

Forest Lodge, Maresfield, Uckfield,
1891, August 3.

WILLIAM NOBLE.

OBSERVATORIES.

EALING.—The second five-foot mirror has been finished and mounted in the telescope. The bad weather has prevented any work beyond a few photographs of a very low Moon and two of

Jupiter with an enlarged image; these are most satisfactory. As far as a bad sky has allowed the mirror to be tested on stars, it is a most excellent one, quite free from the slight fault the first one had. The enlarged image of Jupiter, half an inch in diameter, was found to be over-exposed in 10 seconds, but still much detail is visible on the disc. Two satellites which happened to be near at the time the photographs were taken (August 5th) are visible on the plates, though at the time the sky was not at all good.

PUBLICATIONS.

Pictorial Astronomy *.—We have here another book written for the general reader. Considering the difficulty in providing matter strong enough but not too strong for the mental digestion of a beginner, Mr. Chambers has done his work very well. The illustrations are numerous and fairly good; there is not in all cases that connection between them and the text that could be wished for. There are many mistakes, as is natural in a first edition; a great many points might be made much clearer. The word "perihelion" is used on page 13 correctly, but another word "perigee" is given, and "apogee" is given as the correlative of "aphelion." The statement on page 149 giving a possible diameter of one mile to a fireball is astounding; Young gives ten feet as a limit.

The most serious objection we would take is that the book is not brought up to date; surely something in the history of Astronomy might be said of the great advances that have been made in photographic and spectroscopic astronomy. Still the book is very well done and will, we hardly doubt, prompt many who read it to take a permanent and serious interest in Astronomy.

The Telescope †.—A bad book—superficial, inaccurate, and ill-adapted to the purpose declared in the preface, which we gather to be to guide the "young collector" who may wish to make such a study of celestial objects as a lepidopterist does of insects or a philatelist of stamps. We may perhaps give a few examples of the inaccuracy as a warning. Here are two from the same page (43). Foucault's pendulum experiment is described. Without mentioning anything about latitude, it is stated that "the pendulum slowly

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* 'Pictorial Astronomy for General Readers,' by G. F. Chambers, F.R.A.S., &c. Whittaker and Co., Paternoster Square.

† 'The Telescope: an Introduction to the Study of the Heavens.' By Joseph W. Williams. *The Young Collector Series.* London: Swan Sonnenschein & Co. 1891.

alters its position, and in twenty-four hours will have described a circle in the same direction as the hands of a watch move. This is due to the twisting of the string, for it is easier for that to twist than the heavy bob to alter its course of swing." A more hopelessly confused explanation than that offered in the last sentence it would be difficult to frame. A few lines lower we read—"The stars are placed at infinite and, in the majority of cases, immeasurable distances from us." Overleaf: "Place some oil in a transparent fluid and observe that it floats as a perfect sphere"—nothing said about equal density of oil and fluid: what delightful directions for a "young collector" to make a rather delicate experiment! and what a pity it is to write bad books!

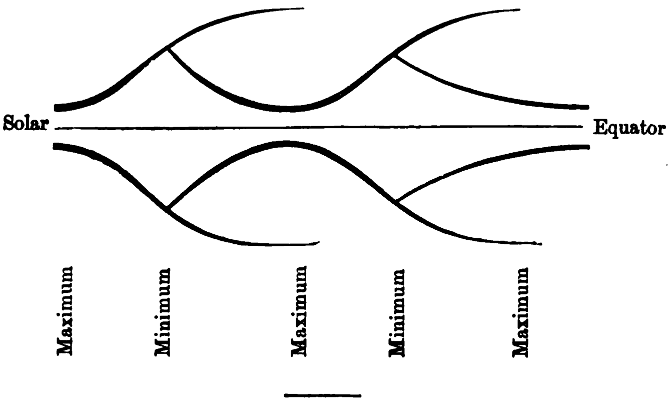
THE SOLAR ECLIPSE OF 1889, JAN. 1.—The Washington University Eclipse Party have published their observations made at Norman, California, on 1889, Jan. 1. The party of 5 (Profs. Pritchett, Nipher, Engler, Charroppin, and Señor Valle) obtained six good negatives, of which 4 are well reproduced in the volume; and there is also given a beautiful picture of the corona which is the "result of a most careful study of all the negatives;" as well as two drawings, one by Prof. Engler agreeing well with the photographs, and one by Señor Valle, who used a disk to shut out the inner corona, and thus saw streamers extending to 8 or 9 diameters from the limb. Contact-observations were made and the position of Norman was subsequently determined by Mr. Keeler, of the Lick Observatory. No essentially new facts were brought to light; but Prof. Nipher takes the opportunity to severely criticise Prof. Bigelow's electrical theory of the solar corona. The expenses of the expedition were defrayed by subscriptions from 35 gentlemen.

MIKROMETRISCHE VERMESSUNG DES STERNHAUFENS Σ 762.—Herr Reinhold Hahn, of the Leipzig Observatory, gives under the above title the results of his differential measures of the group of stars surrounding Σ 762. The position of the principal star of the group is R.A. $6^h\ 34^m\ 51^s$, Dec. $+9^\circ\ 34'$. The magnitudes range from 8 to 12. The plan adopted was to determine the differences of R.A. and Dec. from some one of 10 selected stars whose positions were known from the Leipzig zone catalogue for 1885·0. A mean position for the chief star was deduced from these, and a catalogue of the whole 61 stars formed. Great care has evidently been taken, and particular attention has been given to the thread intervals and value of the screw revolution. A chart of the group is appended. T. L.

NOTES.

Periodicity of Solar Prominences.—Spörer showed in 1882 that the latitude of solar prominences varied in sympathy with the sun-spots. M. A. Ricco has reduced the observations made at Palermo 1880–1890 and finds ample confirmation of this result. Complete observations were made of the chromosphere on 2207 days during this period, and 7663 prominences of more than 30″ were noted.

Just as in the case of spots near minimum, there are two families on the Sun at the same time—one the remains of the dying period, near the equator; and the other in high latitudes, the early members of the new—so in the case of prominences M. Ricco finds two generations overlapping, only here it is the prominences in high latitudes which represent old age, youth being near the equator; and it is at the maximum of activity that we find this coexistence. M. Ricco's diagram, reproduced below, gives a very graphic representation of the facts. His results are given in 'Comptes Rendus,' cxiii. p. 255.



The project for an observatory on Mt. Blanc is not to fail for want of energy or funds. M. Janssen reports that M. Bischoffsheim, Prince Roland Bonaparte, Baron Rothschild, and M. Eiffel have promised their support. The first step is to determine the thickness of the ice-cap, for the Observatory must be built on the solid rock. A horizontal gallery is to be cut in the ice to settle this interesting point.

Photography of Solar Prominences.—Among the visitors to the British Association was Prof. G. E. Hale, Director of the new Kenwood Physical Observatory, who is planning systematic photographic work on the Solar prominences, and has already obtained most encouraging results, some account of which will be found in the Am. Journ. of Sci. for 1891, August; Ast. Nach. 3006, 3037, 3053; and elsewhere. The first result of importance was the brilliant reversal of the lines H and K, "fine bright lines being at the centre of broad dark bands in the atmospheric spectrum." Subsequently Mr. Hale has obtained in all eight lines on the photographs; and measures made since he came to England give for these the following wave-lengths, the suggested origin being indicated by the adjacent columns:—

Prominences (Hale).	Calcium (Rowland).	Hydrogen (Ames).
3968·56	3968·61	....
3933·86	3933·80	....
3888·73	....	....
3970·11	....	3970·25
3889·14	....	3889·15
3835·54	....	3835·6
3798·1	....	3798·0
3770·8	....	3770·7

"Decision must be reserved for the present as to the line 3970·11. The wave-length has been determined from four plates, and its probable error is 0·03; but as the line is not far from where a ghost of H should fall, I cannot be certain that it belongs to the prominence spectrum."

"The line at 3888·73 has not been accounted for. It forms a close double with the hydrogen line at 3889·14, and with it attains as great elevations above the limb as those reached by H and K. The character of the lines, however, is quite different, for while the hydrogen line is wider and slightly diffuse, the line at 3888·73 is very narrow and sharp. I have seen no statement that the hydrogen line has shown any signs of duplicity, and, as Mrs. Huggins has had the kindness to examine the corresponding line in some very sharp photographs of stellar spectra with the same result, we have reason to consider an independent origin probable."

Mr. Hale's instruments are a 12·2-inch equatorial refractor; a spectroscope with 4-inch Rowland grating (14,438 lines to inch), and collimator and telescope of 3¼ inches aperture and 42½ inches focal length, inclined to each other at a constant angle of 25°.

The Variations of the Magnetic Needle.—An important paper by Prof. F. H. Bigelow, already known in connexion with an electrical theory of the corona, appears in the 'Am. Journ. of Science' for September 1891. He has developed a suggestion made by himself in May of last year with most satisfactory results, and finds that the variations of the magnetic needle may be explained by the motions of the Earth through a uniform field of magnetic force *if we take the radiant sunlight as the uniform field of force, directed positive towards the Sun.* This conclusion was deduced from an analysis of the material collected for June 1883 by the International Polar Commission. The monthly means for each hour local time, at thirteen stations, of the horizontal and vertical forces and the declination were reduced to rectangular coordinates; and the differences between the mean and hourly values combined to show the total *deflecting force* at the station. These deflections are considered due to a component coming to the needle from space. "Next a large model was constructed on which these component forces were represented in direction and magnitude. By assigning certain meridians for the hours, and supposing the permanent pole to take up its position from one meridian to another, there was finally collected upon these meridians representing a series of local hour-angles, now referred to the Sun as if the Earth had ceased to rotate on its axis, an exhibition of what exists over the globe at the same instant of time." The model was compared with the theoretical distribution of lines of force over a sphere in a uniform field of magnetic force, as determined by Thomson, Maxwell, and others, with the gratifying result quoted above. And there remains one more important point. "The permanent magnetic condition of the Earth may be principally due to the orbital motion of the Earth through the radiant field of sunlight." We shall look with great interest for the detailed computations which Prof. Bigelow is now undertaking with a view to confirm these important conclusions.

THE ASTROPHOTOGRAPHIC CHART.—We are glad to see that M. Loewy feels the same anxiety which we have at times endeavoured to display lest the scheme for this work should be too ambitious. He has recently requested serious reconsideration of the proposal to systematically observe with meridian instruments all the "guiding stars" and other "étoiles de repère" required for fixing the coordinates of the centres of the separate plates. He points out that this will be a very laborious operation, and by comparison with the revision of Argelander's zones under the auspices of the Astronomische Gesellschaft, now nearly completed, he gives an estimate of the time which will be required for such a work. His very practical suggestions for rendering this labour unnecessary amount essentially to two (*vide* C. R. cxii.):—First, that several plates should be taken together for measurement, a connexion being established between them by micrometric measures of the faint stars photographed on both, in which case we greatly extend the area to be coordinated and make practically certain of including a score of bright stars: and, secondly, to use places of fainter stars already determined, making up for the inaccuracy due to proper motion in the interval between the epoch of observation and 1900 by the large number of such stars available in this way. We should be very glad to see these suggestions carried into effect so as to at least considerably reduce the scheme for meridian observation.

MR. T. W. BACKHOUSE has published, as No. 1 of the "Publications of West Hendon House Observatory," the observations on which were founded his two papers on "The Structure of the Sidereal Universe" (Mon. Not. vol. l. p. 374, and Sid. Mess. vol. ix. p. 337). It is to painstaking observers like Mr. Backhouse that we must look for the accumulation of facts necessary to the unravelling of the complicated problem to which Dr. Huggins so hopefully alluded as the work of the next century. The promise of future material implied in the superscription is gratifying.

WE learn with great pleasure that several members of the "Astronomical Society of the Pacific" have generously subscribed to the "Father Perry Memorial Fund." In order to complete the amount for the new object-glass £140 more is still required, so that further subscriptions will still be very welcome.

MR. W. W. CAMPBELL, of Michigan University, has been appointed Astronomer in the Lick Observatory in the place of Mr. Keeler, who has gone to Allegheny.

PROF. BARNARD'S observation of Encke's Comet, quoted on p. 304, was afterwards corrected. Mr. Hind finds that Backlund's elements with

$$\delta M = +1' \ 15''.0$$

represent the corrected observation within about 10″; and thence deduces the following ephemeris:—

	R.A. h m s	Decl. ° ′	I.
Sept. 5	7 7 10	+34 35·8	1·00
9	7 40 38	33 28·4	1·24
13	8 15 34	31 38·2	1·53
17	8 51 3	29 2·9	1·87
21	9 26 21	25 44·6	2·27
25	9 59 48	21 48·6	2·73
29	10 32 3	+17 23·8	3·27

ERRATA.—P. 286, line 2, *for* 699 *read* 669; p. 289, line 21, *for* a *read* no.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 180. OCTOBER. 1891.

MEETING OF THE ASTRONOMICAL SOCIETY OF THE PACIFIC.

Held at Lick Observatory, 1891, Sept. 5.

The Directors' meeting was held from 5.30 to 6 o'clock, President Pierson presiding.

The minutes of the last meeting were approved.

Twenty-five members were elected.

Mr. Bischoffsheim was elected to life-membership. The membership of the Society is now 420, of whom 44 are life-members.

The meeting of the Society was held in the Library at 7 o'clock, President Pierson in the Chair.

The minutes of the last meeting were approved.

The Secretary announced that 38 presents had been received since last meeting, and the thanks of the Society were voted to the donors.

The following papers were presented:—

a. "Measurement of Jupiter's Satellites by interference methods," by Professor Michelson, of Clark University, Massachusetts.

b. "Enlarged Drawings from the Moon-negatives of the Lick Observatory," by Professor Weinek, Director of the Observatory of Prague.

c. "Catalogue of the Library of the Society," prepared by Otto von Geldern.

d. "Observations of Jupiter and of his Satellites with the 36-inch Equatorial of the Lick Observatory (1888–1890)."

e. "The Observatory of the United States Military Academy at West Point," by Lieut. Harlow, in charge.

The first-named paper only was read by Prof. Campbell, and the meeting adjourned.

After adjournment, the members and their friends were given the use of the 12- and 36-inch equatorials, and made the most of the superb seeing until midnight.

Charles Burckhalter, *Secretary.*

Astronomical Bibliography.

STUDENTS of astronomical history and literature will find a great source of pleasure in the 'Catalogue of the Crawford Library of the Royal Observatory, Edinburgh (497 pp. 4to, in two columns), complied by Lord Crawford and Dr. Copeland and distributed in February last*, as this work differs in many ways from similar catalogues and contains the titles of many scarce publications.

Among astronomical Bibliographies the great work of Lalande and the catalogue of the Pulkova library must always rank in the first line, and they possess distinctive features, by which they supplement each other. Lalande follows strictly the chronological order, which at the time when he compiled his invaluable work, before the great astronomical journals had been started, was perhaps the most natural. The Pulkova Catalogue first divides astronomical publications into two classes, separately published books (libri majores) and dissertations and reprints (libri minores sive dissertationes). Within these two divisions the titles are arranged according to subject, while a good index enables the writings of any author to be picked out without trouble. The catalogue of dissertations is not confined to papers which actually exist in the library in separate copies, but journals and proceedings of academies have been searched and the titles of any papers of more than a passing interest inserted in their proper places. This makes the Pulkova catalogue of great value to anyone who wishes to read up a particular subject.

An attempt to compile a complete astronomical bibliography has been made by the late M. Houzeau together with M. Lancaster, whose great work is still in course of publication. But however much one must admire the patience and diligence of the authors, and however valuable and interesting the historical introduction by M. Houzeau is, it cannot be denied that there are grave deficiencies in this work. Not only have the authors attempted far too much and made the book swell to an inconvenient size, by introducing a great deal of extraneous matter, but in many cases they have evidently not seen a book or paper themselves, and some curious errors have arisen in this way †. Of course it would be impossible for one or two writers to have seen with their own eyes all astronomical publications (unless, indeed, they made a tour of the principal libraries of Europe), but it would have been safer to have indicated in some way the titles which have been copied second hand, as Lalande *always* does. In the absence of any mark by which such titles can be distinguished, a reader cannot place the same confidence in the otherwise very valuable 'Bibliographie astronomique' as he would have a right to do if that precaution had been adopted. In this respect the catalogue of any large library, such as that of the R. Astron. Society, or even a good auction catalogue, such as those of Libri's or Chasles' collections, possess an immense advantage, as one can be absolutely sure that nothing is entered in them on mere hearsay, but that every book or paper has actually been handled by the compiler and (in the case of library catalogues) may be found and consulted in the place indicated. This is of course also the case with the new Edinburgh catalogue, but it is chiefly on account of the many rare books contained in it and the unusually full titles and descriptions of them, that this work will be found of great value to students of astronomical literature.

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* A special one-column edition was printed in only thirteen copies, affording room for additions and notes.

† As an example we may mention in vol. i. p. 877: "Liv den Tycho Brahes." This is *not* Danish, and there never was a book with a title resembling this in the least. The authors have evidently never seen Weistritz's 'Lebensbeschreibung des T. von Brahe,' as they would have found it explained in the preface that it is a translation, not of a Danish *book*, but of two sets of memoirs in Danish periodicals. On the same page and the next one three short papers in journals have been mistaken for separately published books (Nos. 6202, 6203, 6207); the book of Friis (6205) was not published in a second edition.

The library of Dun Echt Observatory, which in 1888 was presented by Lord Crawford to the Royal Observatory, Edinburgh, was collected in the years 1872 to 1888, a good beginning having been made by the acquisition of Babbage's library. A number of very rare works were transferred from the library of the late Lord Crawford at Haigh Hall, Lancashire. The printing of the catalogue was commenced in the spring of 1887 and was finished in the summer of 1889, but new acquisitions were entered while the printing was progressing. It contains between eleven and twelve thousand titles, a great many of which refer to short papers, while, on the other hand, others are titles of serial works of a number of volumes. The catalogue is an alphabetical one, arranged chiefly according to the authors' names, and the titles have been very little contracted, while the number of plates, pages, and unnumbered leaves are carefully indicated. It is needless to say that this information is often very useful to anyone who wishes to know if his own copy of a book is complete *. As regards these particulars the present catalogue gives fuller details than any other work on astronomical bibliography, Scheibel's not excepted. As a means of classifying a number of works which it would otherwise have been difficult to pick out, upwards of twenty subject-headings have been introduced; but when an author's name is distinctly given in works thus catalogued, they appear again under his name. Under the headings of *Academies* and *Observatories* serial publications are given under the name of the town in which the institution in question is situated, and this department is very complete.

The most important subject-heading is that of Comets, which is remarkably extensive, and no pains have been spared to get together as many as possible of the quaint little pamphlets which the terror of comets produced during the seventeenth and part of the eighteenth centuries, especially in Germany. Many of these are no doubt worthless except as curiosities; but as a grain of gold can occasionally be found among a heap of rubbish, it is well that this great collection of pamphlets (second probably only to that at Pulkova) has found a permanent home in the Edinburgh Observatory, particularly as they are becoming very scarce and dealers of late years demand exorbitant prices for them.

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* On p. 146 it should have been stated that the first and only number of vol. xv. of Zach's 'Correspondance Astronomique' is wanting (it is very scarce, the publication having been suddenly stopped by the clerical *Censors* of Genoa).

The literature of the fifteenth and sixteenth centuries has evidently been specially picked out on every occasion, and the catalogue is singularly rich in rare old books from that period. Thus there is a wonderful collection of the ephemerides of Regiomontanus, all extremely scarce and well worth being looked at with veneration, since they played an important part in the great voyages of discovery. In addition to these there is quite a number of books printed before the year 1500 *. Passing to the next century it is deserving of special notice that the library contains a copy of Tycho Brahe's little book on the new star, published in 1573 and so exceedingly rare that neither Lalande, nor Kästner, nor Rudolph Wolf had ever come across it. It is also interesting to see a copy of Tycho's book on the comet of 1577 with the original colophon, showing that the book was complete from the press at Uraniborg in 1588, thirteen years before its author's death †. If Mädler could have seen this copy he would probably not have made the monstrous assertion, that Tycho Brahe's system of the world (which is set forth in this book) was in all probability falsely attributed to him, as it was published after his death in a book of which there is no proof that he was the author! Poor Tycho, who was so proud of his system, just as proud as Mädler was of his own central sun!

It would lead us too far if we were to enumerate all the scarce books described in this catalogue. Naturally modern literature is very fully represented (though here and there a well-known book may be looked for in vain); and here again we find occasion to admire the fulness of the collection by seeing a complete set of Brünnow's 'Astronomical Notices,' twenty-nine in number (the R. A. S. has only 28; it is strange how scarce this journal has become). But the list of Manuscripts (placed together at the end of the volume) deserves being specially mentioned. It contains a number of MSS. from the fourteenth and fifteenth centuries, many of which came from Chasles' collection, as well as some valuable modern ones, such as Carrington's original Transit Circle

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* After a very extensive use of the Catalogue I have only found one single error in it, viz. on p. 237. The book, of which the title is given under the name of Hippocrates, is not by that author, but the last seven pages of the book are taken up by *Hyppocratis libellus de medicorum astrologia.* Lalande made two different books of this very scarce and interesting old book, of which I rejoice in the possession of a copy.

† Compare the writer's *Tycho Brahe,* p. 181.

Ledgers *. Among the most interesting manuscripts are the meteorological diaries kept by various members of the Kirch family at Berlin from 1697 to 1774 with some interruptions. In addition to notes on the weather the successive writers also now and then recorded astronomical phenomena; thus the discovery of the globular cluster M 5 by Gotfried Kirch in 1702 and careful observations of the sixfold tail of the comet of 1744 attest the watchfulness of this family of observers †.

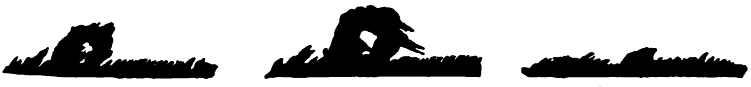
Lord Crawford deserves the thanks of astronomers, not only for having presented this magnificent collection of books to the Edinburgh Observatory, but also for having printed this carefully compiled catalogue of all its treasures. J. L. E. Dreyer.

Notes on some recent Solar Disturbances.

(1) The bright Solar Prominence of 1891, September 10.

The early part of September was marked by a great revival of Solar activity. The largest group of spots witnessed since June 1885 crossed the Sun's disc between Aug. 29 and Sept. 10, in approximate solar latitude 22° N. Its first appearance on the eastern limb was sketched on Aug. 29 in latitude 21° N., and its transit over the western limb was observed both on the screen and in the spectroscope, on Sept. 10, in latitude 22° N.

A comparatively small but compact prominence, of great brightness, was seen through the latter instrument at 0.40 G.M.T. It attained its greatest height, about $35''$ of arc, at 1.50; when it assumed the appearance of four blowpipe-jets intensely bright at the bends. This appeared to be the time of its greatest activity, judging by its dimensions, brightness, and rapidity of change. It was watched until 2.15, when the observer had to leave it. And at his return at 3.5 no prominence was visible; but its place on the limb was distinctly marked by the intensity of all the bright lines which had been observed during its appearance: the prominence as such was extinct. 26 bright lines had been counted in all. And of these the usual lines of the chromosphere, C F *g* and D_3, were so greatly intensified that the most inexperienced observer could not have failed to see the form of the flame by any one of these colours. The well-known lines in the



$1^{h}\,40^{m}$ $1^{h}\,50^{m}$ $3^{h}\,5^{m}$ P.M.

1891, Sept. 10. G.M.T.

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* The Redhill observations of sun-spots were presented by Lord Crawford to the Royal Astronomical Society.

† Compare *Copernicus*, vol. iii. p. 104.

red, 7055 and 6677 of Ångstrom's scale, and D_1 D_2 b_1 b_2 b_3 b_4 and *h*, were seen bright almost quite up to the extreme height of the prominence; while the remaining 13 lines glowed only up to about half the average height of the chromosphere. These were not accurately identified, but 9 of them were seen between λ 5300 and 5400, and probably belong to the group of iron lines in that règion; the other four were seen between the *bb* group and F.

A careful measurement of the spot drawing made earlier in the morning shows that a strong nucleus of a spot group was exactly upon the limb at the time of the spectroscopic observations.

A more detailed account of the group of spots, as observed during its passage, may be given later. But in connection with the bright prominence it must be noted here that the entire group of spots may be divided into two parts—a preceding and a following sub-group. The first of these was not observed on the limb. It was well over it on the morning of the 10th of September. The preceding part of the following sub-group appears to have been the seat of the observed disturbance, and this was the most active centre of the whole group during its passage over the solar disc. Both of the sub-groups were examined with the spectroscope on Aug. 30, Sept. 3, and Sept. 4; when nothing unusual was discovered in the first, while the preceding part of the following sub-group gave evidence of great activity by reversion, distortion, and widening of the C line, on all the three days of observation.

The terrestrial magnetic field may be described as generally more disturbed during the progress of the spot than in the preceding month, and notably at the beginning and end of its course.

But there are no indications of magnetic disturbance accompanying the solar eruptions seen through the spectroscope. Even the brilliant display on the western limb, of the 10th, has left nothing that can be considered a record of itself on the magnetograph curves.

WALTER SIDGREAVES.

Stonyhurst Observatory,
Lancashire.

(2) The Disturbances of 1891, June 17.

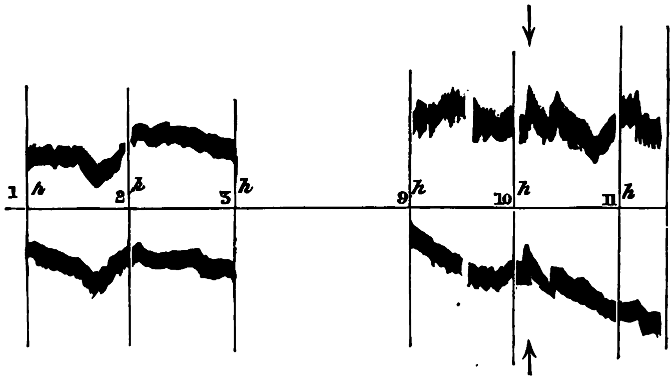
On 1891, June 17, 10^h 9^m Greenwich Civil Time, M. Trouvelot projected the Solar image on a screen, when his attention was suddenly arrested by an "extraordinary luminous appearance such as he had never seen before."

"Close to the western limb was a bright spot subtending an angle of 3° on the limb, greatly surpassing in intensity the most brilliant faculæ he had ever seen. The light was not white as in faculæ, but slightly yellow, very like that of an incandescent lamp just before it acquires its maximum brilliancy. . . . . . A minute later there appeared, a little north of this object, a narrow line of facula, parallel to the limb and at a little distance from it, and subtending an arc of 5° to 6°, shining with the same kind of light but not so brilliantly."

After observing these phenomena for a few minutes, M. Trouvelot adjusted the spectroscope on the region and found two prominences, "one at 281° and the other at 286° to 290°." (No further explanation of these figures is given in C. R., No. 25, p. 1420, but from comparison with the photographs, to be presently mentioned, it is inferred that they are angles from N. point in the direction N.E., S., W.). But it soon became evident that the phenomena were diminishing in brilliancy; which was confirmed on removing the spectroscope and again mounting an ocular. "Là, ou quelques minutes plus tôt etincelait une si éblouissante lumière, on ne voyait rien d'inusité, pas même la plus faible trace de facule." [We imagine that here M. Trouvelot is referring only to the place formerly occupied by the bright light at first noticed; and does not mean that the facula which appeared a minute later had also disappeared: though this is not quite clear from his words. We shall return to this point immediately.] The spectroscope was replaced and the prominences were found to have undergone some change, but to be nearly as bright as before. Only a few lines were reversed, however: C, D^3, F, and G were very bright; but besides these there was only λ 6676·8, *b*, a line in the blue, and perhaps λ 4394·8. Nothing was seen on D^1 and D^2. M. Trouvelot continued to observe the gradually diminishing spectroscopic phenomena till midday; and again, on June 18 from 9^h 20^m till 2^h 35^m, when "le calme est rétabli et toute trace de protubérance éruptive a disparu." In the afternoon of June 17, observations were made quite independently by M. Jules Fényi, at Kalocsa. He refers to the disturbed region (now marked by the facula alone?) as "un groupe de taches en train de se développer à 21° de latitude, qui allait, selon notre calcul, franchir le bord occidental du Soleil à 282° du pôle céleste." But his observations were confined to the neighbouring prominences, whose enormous velocity in the line of sight struck him at once. Careful measurements gave as the value of this velocity 300 to 400 kilometres per second; and the rapid development of the prominence from a height of 111″ to 257″ indicated a velocity of about 500 km. per second transverse to the line of sight. The resultant velocity, even allowing only a moderate component in the third direction, may have at times reached 1000 km. per second! If the appearances are due to matter in actual motion with such velocity as this, it would of course escape from the Sun's attraction.

In concluding his account of the phenomenon, M. Trouvelot compares it to that simultaneously observed by Carrington and Hodgson (Monthly Notices, xx. pp. 13–16) on 1859 Sept. 1, which was accompanied by marked magnetic disturbances; and he asks whether such was also the case on Sept. 17 On examining the Greenwich photographic records, we find a very minute, though unmistakable, disturbance at almost precisely the time noted by Trouvelot. It is not quite clear from his account whether he began observing at 10^h 7^m (10^h 16^m, Paris time), or whether the bright spot suddenly appeared then. But it *is* clear that the disappearance was very rapid, and the whole phenomenon very short-lived. Now at $10^h\ 7^m$, within a very small limit of error, there was a sudden small disturbance of all three magnets (registering declination, horizontal force, and vertical force), as will be seen from the appended diagram for 2 of the elements. The disturbance is smaller than many others on the same day, although the day itself was very quiet; but it differs from others, one of which (at about 3^h) is shown for comparison in its abruptness, which is clearly shown in all three curves. The change in declination is only about 1′, and in H.F. 0·0005 of the whole H.F. We may compare with these the change of 17′ in declination, and 0·0064 of the whole H.F., which took place on 1859 Sept. 1, simultaneously with the Carrington-Hodgson disturbance, and the tremendous magnetic storm which went on for some days, during which the changes were much larger still; and it becomes obvious that the phenomena of June 17 were of an entirely different order of magnitude.

Declination.



Horizontal Force.

Magnetic Disturbance, 1891 Sept. $17^d\ 10^h\ 7^m$.

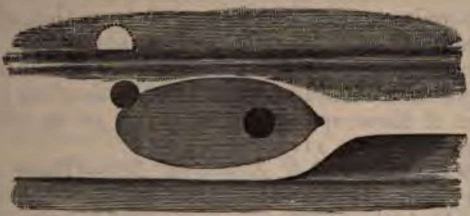
Two photographs of the Sun were taken at Greenwich curiously near the time of M. Trouvelot's observation—one at $10^h\ 19^m\ 53^s$ G.C.T., and another at $10^h\ 51^m\ 53^s$. Both show the facula described by M. Trouvelot, but there is no trace of anything extraordinary; nor, indeed, should there be, according to M. Trouvelot's account, for the brilliant region had disappeared within a few minutes after $10^h\ 7^m$. H. H. TURNER.

Notes on Jupiter.

The Red Spot.—I observed the red spot pass the central meridian of Jupiter on Aug. 28 at $13^h 51^m$ and on Sept. 12 at $11^h 11^m$, so that it followed Marth's zero meridian about 6 minutes. Comparing the observation on Sept. 12 with one made here on Sept. 1, 1890, at $9^h 35^m$, I find the mean rotation-period

$$9^h 55^m 40^s{\cdot}9 \text{ (909 rotations).}$$

The motion of this object appears therefore to have been slightly retarded during the last twelve months. To my eye the major axis of the spot is less than it formerly was. There is a small black speck on its *f.* end, and the red hue is now very obvious.



Red Spot and Sat. III. and its shadow. 1891, Sept. 10, about 10^h.
10-in. reflector; power 252.

On Sept. 10 Sat. III. and its shadow were projected on the region of the red spot; the shadow was black, while the satellite itself was brown and placed on the *s.p.* border of the spot.

Markings S. of the Red Spot.—Amid the faint belts outlying the red spot there are dark and bright markings. The most conspicuous of the former is an elliptical dark spot or thickening of a belt in about S. lat. 30°. I noticed this on the C.M. on Sept. 4 $8^h 48^m$, Sept. 11 $9^h 25^m$, and Sept. 15 $12^h 35^m$. It is about 36 minutes crossing the C.M., so it extends over nearly 22° in longitude. This is the same object to which Mr. A. S. Williams called attention in the 'Monthly Notices,' June 1890, p. 521, and in July 1890 its latitude was diverted several degrees northwards owing to the repelling influence exerted by the red spot. Mr. Williams noted the former object central on March 28, 1890, at $17^h 20^m$, and on comparing this observation with mine on Sept. 15 last, I find the resulting period

$$9^h 55^m 18^s{\cdot}3 \text{ (1296 rotations).}$$

It gains 22·6 on the red spot during a single revolution of the latter, and this is equivalent to 54·6 per day. Its conjunctions with the red spot must therefore occur at intervals of about 655 days. It now follows the red spot $3^{h}\ 54^{m}$, so that the next conjunction will take place on about May 30, 1892.

In the same latitude there are two brilliant spots separated by some 35°, and shining on the S. side of the faint dark belt. The *f.* spot of these was on C.M. as follows:—

1891.	h	m	
Aug. 21	13	2	In conjunction with Red Spot.
28	13	41	Precedes Red 10 min.
Sept. 2	12	42	
12	10	48	Precedes Red 23 min.

I find the period is

$$9^{h}\ 55^{m}\ 18^{s}{\cdot}2 \text{ (53 rotations),}$$

which differs only one-tenth of a second from that of the spot previously alluded to, and is doubtless situated in the same current. In the winter of 1880–1 I found a period of $9^{h}\ 55^{m}\ 17^{s}{\cdot}9$ for a dark spot in this latitude, and Mr. Williams gives $9^{h}\ 55^{m}\ 17^{s}{\cdot}1$ as the mean of three spots seen in S. lat. 30° in 1887 ('Zenographical Fragments,' p. 111).

Dark spots on belt N. of N. Equatorial Belt.—These are numerous and very distinct; they are situated on the S. half of a double belt in about N. lat. 25°. They are of special interest, from the fact that their motion is swifter than that of any other Jovian features. One of the chief spots was observed on the C.M. as follows:—

1891.	h	m	1891.	h	m
Aug. 21	12	41	Sept. 11	9	39
28	11	41	13	10	46
Sept. 4	10	44	15	11	52

The resulting period is

$$9^{h}\ 49^{m}\ 27^{s}{\cdot}2 \text{ (61 rotations),}$$

or $6^{m}\ 13^{s}{\cdot}7$ less than the red spot, so that the daily loss of the latter amounts to $15^{m}\ 3^{s}{\cdot}4$. The conjunctions of the two spots will therefore occur at intervals of a little over 39 days. On Sept. 15, at 13^{h}, there were five of these dark N. temperate spots visible at the same time; they were strung like beads on the S. half of the belt. Mr. A. S. Williams has also been observing these features, and has noted their rapid motions. He finds that some of these spots move more rapidly than others.

But, rapid as these spots are moving, their velocity is less than that of some dark spots which broke out in the same latitude in the autumn of 1880, and gave a rotation-period of $9^{h}\ 48^{m}$. These phenomena were, however, somewhat evanescent, for the dark material forming them dispersed itself longitudinally and soon formed a new dark belt on the planet. Has the rapid motion which affected this latitude in 1880 been retarded to the extent of some 8 seconds annually, so that the period of $9^{h}\,48^{m}$ in 1880 has been increased to $9^{h}\,49^{m}\,27^{s}{\cdot}2$ in 1891, or must we regard the present outbreak as new and the great proper motion as merely of temporary character? From what we know of the behaviour of the markings, it is highly probable that there has been a gradual slackening of pace going on in the dark material of the belt since 1880. The red spot and equatorial markings show a retardation, and the same peculiarity affects some other features. It is very singular that in the region of N. lat. 25° the spots move with such exceptional speed.

Bright spots on N. side, N. Equatorial Belt.—A very pronounced feature of this character was on the C.M. on Aug. 28 $10^{h}\,53^{m}$, and there was a small black spot closely *p.* and *f.* I observed the same object on Sept. 8, 1890, $8^{h}\,2^{m}$, when it was at mid-transit, so the spot has a mean period of

$$9^{h}\,55^{m}\,38^{s}{\cdot}3 \text{ (831 rotations),}$$

and only $2^{s}{\cdot}6$ less than the red spot.

There are other similar spots in this latitude (about N. 12°). I saw one of these objects on Nov. 27, 1884, and frequently re-observed it in 1885 and 1886, and it is probably identical with the one now visible; but I cannot prove this, as my observations have not been continuous. It now precedes the red spot about 3 hours.

White Equatorial Spots.—A number of bright spots have been observed on the equatorial side of the great S. belt. The following are the times of observed transits:—

1891.	h	m
Aug. 28	10	23*
Sept. 2	11	47
3	10	53
4	9	45*
4	10	47

These are different spots, except in the case of the two marked with an asterisk. I have not been able to secure any later observations of these objects, and must reserve the discussion of their periods for a future communication.

I send you these notes in the hope that other observers will compare results and contribute something to your pages on the same subject.

Bristol, 1891, Sept. 20. W. F. DENNING.

Observations at Morrison Observatory on the Surface-markings of Jupiter, from August to December 1890.

Date.	G.M.T.	Phenomena and Explanatory Notes.	1–5 Weight.
1890.	h m		
Aug. 14	14 47	Red Spot at centre of disc. Two fine round black spots north of Red Spot, much resembling the shadows of satellites. At first they were taken to be shadows.	2½
Aug. 21	15 31·3	Centre of Red Spot at centre of disc. The transit of the spot was *most carefully observed*, both by the method of equal segments, as in former years, and by actual micrometer measures. Seeing very good. Both the transit of centre and of following end observed. The two fine sharp black spots on north edge of north equatorial belt have the same relative position to each other. The *following one* of these spots is very little *preceding* the centre of Red Spot. The dimensions and outline of the Red Spot remain about the same as they showed themselves twelve years ago. It has still a pale red hue, which comes out beautifully in a favourable atmosphere.	4
Aug. 26	14 42	Centre of Red Spot at centre of disc. Seeing good.	3
Aug. 28	16 20	Centre at centre of disc. Fine seeing. Drawing made of Red Spot. Observed both centre and following end at transit.	4
Sept. 2	15 24	Centre of Red Spot at centre of disc. Outline of spot distinct. The larger and *preceding* of the two black spots seen Aug. 14–21 was at first taken for the *shadow of a satellite*, and on this hypothesis micrometer measures were made of its distance from the central line of the disc, perpendicular to the rotation axis of planet. This distance was found to be $2''\!\cdot96$ north of the central line.	3
Sept. 11	......	In the attempt to obtain transit of centre of Red Spot, due about $17^{h}\!\cdot8$ G.M.T., I was frustrated by the apparent *swelling* or *protuberance* on the belt, south preceding Red Spot, and in apparent contact with Red Spot. This seems to be the dark spot mentioned by Mr. A. Stanley Williams ('Observatory,' Nos. 165, 166), Having passed under the Red Spot it is again emerging.	
Sept. 16	16 56	Red Spot at centre of disc. The precision of this observation is somewhat impaired by the knot or protuberance on the belt *s.p.* the Red Spot. It is not yet clear of the Red Spot. Seeing not very favourable, yet great care was taken to obtain a correct transit.	3
Sept. 17	12 50	Red Spot at centre. Outlines of spot distinct; colour pale red. The shadow of I. was on the disc.	3

Date.	G.M.T.	Phenomena and Explanatory Notes.	1–5. Weight.
1890.	h m		
Sept. 19	14 23	Centre of Red Spot at central meridian. Very clear, yet images not good. The two round black spots on north margin of north equatorial belt very plain to-night, and the *following one* preceded the preceding end of Red Spot by a quantity scarcely appreciable.	3
Sept. 28	16 48	Red Spot at centre of disc. Seeing very poor, yet the two round black spots on north margin of north equatorial belt were distinctly seen in the same position as on 19th.	2
Oct. 4	……	Two fine round black spots, resembling shadows of satellites, were observed on the north edge of the north equatorial belt, and these are *entirely distinct* from the pair of black spots just north of the Red Spot. The *following one* of *this pair* of spots passed the central meridian of the planet at $14^h\,29^m$ G.M.T. Though carefully looked for, *this pair* was never again seen. Perhaps one of these was the spot mentioned by Mr. Waugh, 'Observatory,' No. 167.	
Oct. 6	13 33	Red Spot at centre. Seeing was very good, and the Red Spot was nearly central when the planet passed the local meridian. The two round black spots on N. margin of N. equatorial belt were in their place as usual. The interval between them, carefully measured by the micrometer, was found to be $6''\cdot8$. The following one of this pair follows the preceding end of Red Spot by a *very small* interval. The knot or swell on the belt south of Red Spot is now fairly clear of Red Spot, and makes a conspicuous feature in that S. belt.	4
Oct. 8	15 12	Red Spot at centre. Images steady; definition good. The two black spots preserve the same appearance and relative position to each other and to the Red Spot. The dark knot or swell in the belt *s.p.* Red Spot quite prominent.	3·5
Oct. 11	12 45	Centre of Red Spot at centre. Seeing very fine. The outline of Red Spot was plain and distinct as in former years, yet its colouring is much reduced. The pair of black spots very distinct, with no appreciable change of position. The belts all very sharp, and tinged with red very deeply. The knot or swell on the belt *s.p.* Red Spot has receded still further from Red Spot.	4
Oct. 16	11 50	Red Spot at centre. Observation made in early twilight and just before planet came to meridian. The knot or belt *s.p.* is still further receding from Red Spot.	4

ate.	G.M.T.	Phenomena and Explanatory Notes.	1–5. Weight.
1890.	h m		
ct. 18	13 29	Centre at centre. Seeing tolerable, at intervals very good. The *following* one of black spots is still very little *following* the preceding end of Red Spot. The knot in belt *s.p.* Red Spot still recedes very slowly from Red Spot, and is much less conspicuous in the belt.	3·5
Oct. 23	12 35	Centre at centre. Seeing excellent. Transit of centre and following end observed. During the observation the shadow of Sat. II. was situated on the S. margin of the N. equatorial belt, *between* the pair of black spots, thus affording a *rare opportunity* to compare the spots with the shadow. There was a difference in the intensity of the blackness in favour of the shadow. There is no change whatever in these spots, except that they *may be* a little elongated in the direction of the belt. They are very *sharp* to-night. The dark knot in belt *s.p.* Red Spot quite plain, and is slowly receding towards the limb of the planet.	4
Oct. 25	14 12	Centre of Red Spot at centre. Seeing poor.	2·5
Oct. 30	13 19	Centre of Red Spot at centre. Seeing very variable.	3
Nov. 4	12 30	Centre of Red Spot at centre. Seeing excellent. Planet near meridian. Outline of spot very distinct. The pair of black spots still keep their place—following one is still a very little *following* the *preceding* end of Red Spot. They are still very nearly round, and much resemble the shadows of satellites.	4
Nov. 21	11 34	Centre of Red Spot at centre. Observed both centre and following end. Outlines sharp and plain.	4
Nov. 26	10 44	Red Spot at centre. Observed about sunset and planet near meridian. Seeing good and spot quite plain.	3·5
Dec. 3	11 30	Red Spot at centre of disc. Seeing very variable; planet has just passed meridian. Observation fair. The pair of black spots seen as usual, the following one just *following* the preceding end of red spot.	3

The foregoing observations and notes were drawn off fro
observing-book several months ago; but serious family aff
has delayed their transmission for publication till now. P
some of the notes may have a value during the present opp

The rotation time of the planet, as deduced from the o
tions of the Red Spot, was computed by Prof. H. S. Prit
Washington University, St. Louis.

For this purpose the observations were reduced to Ge. Long. 305° and to Ge. Dist. 4·80:

Correction for Long. were of the form $1^{m}\cdot654\ (305^{\circ}-L)$,
„ Light Eq. „ „ $8\cdot32\ (4\cdot80-\Delta)$,

where L is the longitude and Δ the distance at any epoch.

The equations of condition, involving 269 rotations of the planet, were 21 in number, and of the following form—

$$K+N(595^{m}\cdot7+x)=T_1-T_0,$$

where K = error of observation of Aug. 14, 1890.
N = number of observations between that date and time T_1.
x = correction to the assumed rotation, $595^{m}\cdot7$.
T_1 = corrected time of transit of spot at any date.
T_0 = „ „ „ on Aug. 14.

Treated by the method of least squares, these equations gave the normals

$$21\cdot0\,K+\ \ 2679\cdot0\,x+\ \ 148\cdot1=0,$$
$$2679\cdot0\,K+462985\cdot0\,x+28344\cdot1=0,$$

where $$x=-0^{m}\cdot077226,$$

and therefore the rotation time = $9^{h}\ 55^{m}\ 37^{s}\cdot366\pm0^{s}\cdot378$.

This result would seem to indicate that the retrograde motion of the Red Spot has at least *slackened* within the last two years. It must be admitted, however, that the probable error of a single observation is *large* and also some of the residuals. But this was to be expected, as at least half of the observations were made under unfavourable conditions. C. W. Pritchett.

July 1891.

[We have printed the above notes after some hesitation. It is not within our province to publish original observations, which are much more accessible if printed in the publications regularly devoted to them. But it may interest our readers to compare the above notes with their own observations. In several particulars they illustrate more especially the way *not* to make notes. Compare, for example, the variety and inaccuracy of the expressions used for describing the transit of the Red Spot across the central meridian.—Eds.]

An Astronomer's Work in a Modern Observatory *.

The work of Astronomical Observatories has been divided into two classes, viz. Astrometry and Astrophysics. The first of these relates to Astronomy of precision, that is to the determination of the positions of celestial objects; the second relates to the study of their physical features and chemical constitution.

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* From the 'Proceedings of the Royal Institution of Great Britain,' May 29, 1891.

Some years ago the aims and objects of these two classes of observatories might have been considered perfectly distinct, and, in fact, were so considered. But I hope to show that in more recent years their objects and their processes have become so interlaced that they cannot with advantage be divided, and a fully equipped modern observatory must be understood to include the work both of *Astrometry* and *Astrophysics*.

In any such observatory the principal and the fundamental instrument is the transit-circle. It is upon the position in the heavens of celestial objects, as determined with this instrument or with kindred instruments, that the whole fair superstructure of exact astronomy rests; that is to say, all that we find of information and prediction in our nautical almanacs, all that we know of the past and can predict of the future motions of the celestial bodies.

Here is a very small and imperfect model, but it will serve to render intelligible the photograph of the actual instrument which will be subsequently projected on the screen. [Here the lecturer described the adjustments and mode of using a transit-circle.]

We are now in a position to understand photographs of the instrument itself. But first of all as to the house in which it dwells. Here, now on the screen, is the outside of the main building of the Royal Observatory, Cape of Good Hope. I select it simply because, being the observatory which it is my privilege to direct, it is the one of which I can most easily procure a series of photographs. It was built during the years 1824–28, and like all the observatories built about that time, and like too many built since, it is a very fair type of most of the things which an observatory should not be. It is, as you see, an admirably solid and substantial structure, innocent of any architectural charm, and, so far as it affords an excellent dwelling-place, good library accommodation, and good rooms for computers, no fault can be found with it. But these very qualities render it undesirable as an observatory. An essential matter for a perfect observatory should be the possibility to equalize the internal and the external temperature. The site of an instrument should also be free from the immediate surroundings of chimneys or other origin of ascending currents of heated air. Both these conditions are incompatible with thick walls of masonry and the chimneys of attached dwelling-houses, and therefore, so as far as possible, I have removed the instruments to small detached houses of their own. But the transit-circle still remains in the main building, for, as will be evident to you, it is no easy matter to transport such an instrument.

The two first photographs show the instrument, in one case pointed nearly horizontally to the north, the other pointed nearly vertical. Neither can show all parts of the instrument, but you can see the massive stone piers, weighing many tons each, which, resting on the solid blocks 10 feet below, support the pivots. Here are the counter-weights which remove a great part of the weight of the instrument from the pivots, leaving only a residual pressure sufficient to enable the pivots to preserve the motion of the instrument in its proper plane. Here are the microscopes by which the circle is read. Here the opening through which the instrument views the meridian sky. The observer's chair is shown in this diagram. His work appears to be very simple, and so it is, but it requires special natural gifts—patience and devotion, and a high sense of the importance of his work—to make a first-rate meridian observer. Nothing apparently more monotonous can be well imagined if a man is "not to the manner born."

Having directed his instrument by means of the setting-circle to the required altitude, he clamps it there and waits for the star which he is about to observe to enter the field. This is what he sees. [Artificial transit of a star by lantern.]

As the star enters the field it passes wire after wire, and as it passes each wire he presses the key of his chronograph and records the instant automatically. As the star passes the middle wire he bisects it with the horizontal web, and again similarly records on his chronograph the transit of the star over the remaining webs. Then he reads off the microscopes by which the circle is read, and also the barometer and thermometer, in order afterwards to be able to calculate accurately the effect of atmospheric refraction on the observed altitude of the star; and then his observation is finished. Thus the work of the meridian observer goes on, star after star, hour after hour, and night after night; and, as you see, it differs very widely from the popular notion of an astronomer's occupation. It presents no dreamy contemplation, no watching for new stars, no unexpected or startling phenomena. On the contrary, there is beside him the carefully prepared observing-list for the night, the previously calculated circle setting for each star, allowing just sufficient time for the new setting for the next star after the readings of the circle for the previous observation.

After four or five hours of this work the observers have had enough of it; they have, perhaps, observed fifty or sixty stars, they determine certain instrumental errors, and betake themselves to bed, tired, but (if they are of the right stuff) happy and contented men. At the Cape we employ two observers, one to read the circle and one to record the transit. Four observers are employed, and they are thus on duty each alternate night. Such is the work that an outsider would see were he to enter a working meridian observatory at night, but he would find out if he came next morning that the work was by no means over. By far the largest part has yet to follow. An observation that requires only two or three minutes to make at night, requires at least half an hour for its reduction by day. Each observation is affected by a number of errors, and these have to be determined and allowed for. Although solidly founded on massive piers resting on the solid rock, the constancy of the instrument's position cannot be relied upon. It goes through small periodic changes in Level, in Collimation, and in Azimuth, which have to be determined by proper means, and the corresponding corrections have to be computed and applied; and also there are other corrections for refraction, &c., which involve computation and have to be applied. But these matters would fall more properly under the head of a special lecture upon the transit instrument. I mention them now merely to explain why so great a part of an astronomer's work comes in the daytime, and to dispel the notion that his work belongs only to the night.

One might very well occupy a special lecture in an account of the peculiarities of what is called personal equation—that is to say, the different time which elapses for different observers between the time when the observer believes the star to be upon the wire and the time when the finger responds to the message which the eye has conveyed to the brain. Some observers always press the key too soon, some always too late. Some years ago I discovered, from observations to which I will subsequently refer, that *all* observers press the chronograph key either too soon for bright stars or too late for faint ones.

Other errors may, and I am sure do, arise both at Greenwich and the Cape from the impossibility of securing uniformity of outside and inside temperature in a building of strong masonry. The ideal observatory should be solid as possible as to its foundations, but light as possible as to its roof and walls—say, a light framework of iron covered with canvas. But it would be undesirable to cover a valuable and permanent instrument in this way.

But here is a form of observatory which realizes all that is required, and which is eminently suited for permanent use. The walls are of sheet iron, which readily acquire the temperature of the outer air. The iron walls are protected from direct sunshine by wooden louvres, and small doors in the iron walls admit a free circulation of air. The revolving roof is a light framework of iron covered with well-painted *papier mâché*.

The photograph now on the screen shows the interior of the observatory, and this brings me to the description of observations of an entirely different class. In this observatory the roof turns round on wheels, so that any part of the sky can be viewed from the telescope. This is so because the instrument in this observatory is intended for purposes which are entirely different from those of a transit-circle. The transit-circle, as we have seen, is used to determine the *absolute* positions of the heavenly bodies; the heliometer to determine with greater precision than is possible by the absolute method the *relative* positions of celestial objects.

To explain my meaning as to absolute and relative positions—It would, for example, be a matter of very little importance if the absolute latitude of a point on the Royal Exchange or the Bank of

England were one tenth of a second of arc (or ten feet) wrong in the maps of the Ordnance Survey of England—that would constitute a small *absolute* error common to all the buildings on the same map of a part of the city, and common to all the adjoining maps also. Such an error, regarded as an *absolute* error, would evidently be of no importance if every point on the map had the same absolute error. There is no one who can say at the present moment whether the absolute latitude of the Royal Exchange—nay, even of the Royal Observatory, Greenwich—is known to ten feet. But it would be a very serious thing indeed if the relative positions on the same map were ten feet wrong *here* and *there*. For example, if of two points marking a frontage boundary on Cornhill one were correct, the other ten feet in error—what a nice fuss there would be! what food for lawyers! what a bad time for the Ordnance Survey Office! Well, it is just the same in Astronomy.

We do not know, we probably never shall know with certainty, the absolute places of even the principal stars to $\frac{1}{10}$th of a second of arc. But $\frac{1}{10}$th of a second of arc in the measure of some relative position would be fatal. For example, in the measurement of the Sun's parallax an error of $\frac{1}{10}$th of a second of arc means an error of 1,000,000 miles, in round numbers, in the Sun's distance; and it is only when we can be quite certain of our measures of much smaller quantities than $\frac{1}{10}$th of a second of arc, that we are in a position to begin seriously the determination of such a problem as that of the distances of the fixed stars. For these problems we must use differential measures, that is measures of the relative positions of two objects. The most perfect instrument for such purposes is the heliometer.

Lord McLaren has kindly sent from Edinburgh, for the purposes of this lecture, the parts of his heliometer which are necessary to illustrate the principles of the instrument.

This instrument is the same which I used on Lord Crawford's expedition to Mauritius, in 1874. It was also kindly lent to me by Lord Crawford for an expedition to the Island of Ascension to observe the opposition of Mars, in 1877. In 1879, when I went to the Cape, I acquired the instrument from Lord Crawford and carried out certain researches with it on the distances of the fixed stars.

In 1887, when the Admiralty provided the new heliometer for the Cape Observatory, this instrument again changed hands. It became the property of Lord McLaren. I felt rather disloyal in parting with so old a friend. We had spent so many happy hours together, we had shared a good many anxieties together, *and we knew each other's weaknesses so well.* But my old friend has fallen into good hands and has found another sphere of work.

The principle of the instrument is as follows. [The instrument was here explained.]

There is now on the screen a picture of the new heliometer of the Cape Observatory, which was mounted in 1887, and has been in constant use ever since. It is an instrument of the most refined modern construction, and is probably the finest apparatus for refined measurement of celestial angles in the world.

[Here were explained the various parts of the instrument in the relation to the model, and the actual processes of observation were illustrated by the images of artificial stars projected on a screen.]

Here, again, there is little that conforms to the popular idea of an astronomer's work; there is no searching for objects, no contemplative watching, nothing sensational of any kind. On the contrary, every detail of his work has been previously arranged and calculated before-hand, and the prospect that lies before him in his night's work is simply more or less of a struggle with the difficulties which are created by the agitation of the star images, caused by irregularities in the atmospheric refraction. It is not upon one night in a hundred that the images of stars are perfectly tranquil. You have the same effect in an exaggerated way when looking across a bog on a hot day. Thus, generally, as the images are approached, they appear to cross and recross each other, and the observer must either seize a moment of comparative tranquility to make his definitive bisection, or he may arrive at it by gradual approximations till he finds that the vibrating images of the two stars seem to pass each other as often to one side as to the other. So soon as such a bisection has been made the time is recorded on the chronograph, then the scales are pointed on and printed off, and so the work goes on, varied only by reversals of the segments and of the position-circle. Generally, I now arrange for 32 such bisections, and these occupy about an hour and a half. By that time one as had about enough of it, the nerves are somewhat tired, so are the muscles of the back of the neck, and, if the observer is wise, and wishes to do his best work, he goes to bed early and gets up again at two or three o'clock in the morning, and goes through a similar piece of work. In fact this must be his regular routine night after night, whenever the weather is clear, if he is engaged, as I have been, on a large programme of work on the parallaxes of the fixed stars, or on observations to determine the distance of the Sun by observations of minor planets.

I will not speak now of these researches, because they are still in progress of execution or of reduction. I would rather, in the first place, endeavour to complete the picture of a night's work in a modern observatory.

We pass on to celestial photography, where astrometry and astrophysics join hands. Here on the screen is the interior of one of the new photographic observatories, that at Paris. [Brief description.]

Here is the exterior of our new photographic observatory at the Cape. Here is the interior of it, and the instrument. [Brief description.]

The observer's work during the exposure is simply to direct the telescope to the required part of the sky, and then the clockwork *nearly* does the rest—but not quite so. The observer holds in his hand a little electrical switch with two keys; by pressing one key he can accelerate the velocity of the driving-screw by about 1 per cent., and by pressing the other he can retard it 1 per cent. In this way he keeps one of the stars in the field always perfectly bisected by the cross wires of his guiding telescope, and thus corrects the small errors produced partly by changes of refraction, partly by small unavoidable errors in cutting the teeth of the arc into which the screw of the driving-shaft of the clockwork gears.

The work is monotonous rather than fatiguing, and the companionship of a pipe or cigar is very helpful during long exposures. A man can go on for a watch of four or five hours very well, taking plate after plate, exposing each, it may be, forty minutes or an hour. If the night is fine a second observer follows the first, and so the work goes on the greater part of the night. Next day he develops his plate and gets something like this. [Star cluster.]

Working just in this way, but with the more humble apparatus which you see imperfectly in the picture now on the screen, we have with a rapid rectilinear lens by Dallmeyer of 6 inches aperture photographed at the Cape during the past six years the whole of the southern hemisphere from 20° of south declination to the south pole.

The plates are being measured by Professor Kapteyn, of Groningen, and I expect that in the course of a year the whole work containing all the stars to 9½ magnitude (between 200,000 and 300,000 stars) in that region will be ready for publication. This work is essential as a preliminary step for the execution in the southern hemisphere of the great work inaugurated by the Astrophotographic Congress at Paris in 1887, the last details of which were settled at our meeting at Paris in April last. What we shall do with the new apparatus perhaps I may have the honour to describe to you some years hence, after the work has been done.

David Gill.

[To be continued.]

Spectrum of β Lyræ.

[From Astr. Nachr. No. 3051.]

The spectrum of the variable star β Lyræ is unlike that of any other star hitherto examined. With the aid of Mrs. M. Fleming and Miss A. C. Maury a careful study has been made of twenty-nine photographs of this object. These photographs form part of the Henry Draper Memorial. The images on four other plates were too indistinct to be used and were not included in the following discussion. The spectrum is traversed by broad dark bands due to hydrogen, and also by other lines characteristic of many stars in the constellation of Orion, and forming that division of the first type which is designated as B in the Draper Catalogue. But besides these several bright lines are visible which change their positions. The most conspicuous of them have the approximate wave-lengths 486, 443, 434, 410, 403, and 389. The first, third, fourth, and sixth of these apparently coincide with the hydrogen-lines F, G, *h*, and *α*. The others are two of the most marked of the Orion lines. The bright lines sometimes have a slightly greater wave-length than the corresponding dark lines, so that the latter sometimes appear to have bright edges on the less refrangible side, while at other times the reverse is the case.

It is obviously desirable to trace any connection which may exist between these changes and the variations in the brightness of the star, the principal minima of which occur at regular intervals of about $12^d\ 22^h$. There are two maxima occurring at $3^d\ 5^h$ and $9^d\ 16^h$ after the principal minimum, and an intermediate minimum following it $6^d\ 11^h$. Of the eleven plates in which the bright lines had a diminished wave-length it was found that all had been taken during the second half of the period of variation, that is, after the second minimum, and more than $6^d\ 11^h$ after the principal minimum. The fourteen plates taken during the first half of the period all showed an increase in wave-length of the bright lines, that is, the dark lines appear bright on the side towards the red. There are, however, three exceptions, plates at $6^d\ 13^h$, $7^d\ 12^h$, and at $11^d\ 11^h$ show an increased instead of a diminished wave-length. A re-examination of these three plates showed that the deviation of the lines was not very marked, and two other plates taken near the two minima showed a tendency of the lines to occupy an intermediate position, and sometimes apparently to fall on the dark lines so as to nearly disappear.

Since the observations extend over more than four years, or 130 periods of variation of the star, this latter period must coincide, or at least agree very closely, with the period of change in the lines. It seems probable that they are due to the same cause. It should be noticed, however, that bright lines are not visible in the spectra of other variable stars of short period. The spectrum of S Monocerotis is of the Orion type, with dark lines resembling those in the spectrum of β Lyræ; ζ Geminorum, X Sagittarii, V Sagittarii, η Aquilæ, and δ Cephei have spectra of the second type, and the spectrum of T Vulpeculæ is intermediate between the first and second types.

The actual changes in the spectra when studied in detail are much more complicated than has been stated above, and show a variety of intermediate phases, and changes in the dark as well as in the bright lines. In some of the photographs several of the bright lines appear to be double. Micrometric measures are now in progress, additional photographs are being taken, and a complete study of the whole will be made.

The most natural explanation of the motion of the bright lines is that the object emitting them is revolving in a circular orbit having a period of $12^d\ 22^h$. The maximum velocity is approximately 300 English miles (500 km.). The corresponding minimum value of the radius of the circle would be about 50,000,000 miles. Perhaps this object is a close binary resembling β Aurigæ, but with components having unlike spectra. The phenomena may also be due to a meteor stream, or to an object like our Sun revolving in $12^d\ 22^h$ and having a large protuberance extending over more than 180° in longitude. The occasional doubling of the lines would then be due to both ends of the protuberance being visible at the same time, one receding, the other approaching. The variation in light may be caused by the visibility of a larger or smaller portion of this protuberance.

Harvard College Observatory,
Cambridge, Mass.,
1891, June 29.

EDWARD C. PICKERING.

Selenographical Notes.

MACROBIUS.—This fine deep ring-plain is remarkable for its regularity of form and freedom from breaks in the continuity of its circular border, of which there are only two of any importance. One is due to the brilliant crater A on the crest of the eastern wall, and the other to the presence of a curious depression on the north. A resembles, though on a slightly larger scale, the bright ring on the north-east border of Julius Cæsar. Both the latter and A are at least 4 miles in diameter and are similarly associated with the summit ridge of the circumvallation, of which they appear to be expansions, alike in width and brightness. The great depth (some 13,000 feet) of the floor of Macrobius below the border involves a considerable part of the interior in shadow, even when the morning Sun has attained an altitude of 8° and more. At about this stage of illumination, however, the best view is obtained of the wall and its surroundings and especially of the details of the eastern border. The steep crest of this is very narrow and uniform and contrasts strongly in brilliancy with the slope beneath it. It evidently stands at a high angle with the horizontal, perhaps 50° or more, while the latter runs down to the floor at a much more moderate and a constantly decreasing inclination. There is a well-marked valley separating this upper ridge from the remainder of the inner slope, which may be readily traced under morning light in the form of a dark streak running concentrically with the border from the north side of the crater A round the eastern and north-eastern quarter of the ring up to the place on the north where the depression, just referred to, causes an apparent interruption in the continuity of the wall. This terrace, if it may be so termed, appears again on the south side of the break and may be followed under an evening Sun under the crest of the western wall up to the extreme southern end of the formation. .The south-eastern section of the border appears to be devoid of terraces, though both this and the inner slope generally has a streaky aspect which is probably due to concentric depressions much shallower and narrower than that immediately beneath the summit ridge.

Schmidt shows two minute craters on the site of the gap in the northern wall, Mädler a curved ridge connected with a triangular-shaped plateau abutting on the edge of the formation, and Neison Mädler's curved ridge and plateau, with a semicircular depression upon it outside the wall, and a short ridge, hard by, on the floor. None of these representations agree with what may be observed when the W. long. of the morning terminator is about 28° and under other suitable conditions of illumination. At 9^h on May 12, this year, with a power of 350 on my 8½-in. Calver-Reflector, the gap was occupied by a dusky circular ring, nearly double the diameter of Macrobius A, enclosing a heart-shaped depression filled with black shadow. The ring projects for some distance on the floor and the inner slope of the north wall abuts on its eastern flank. On the west it is connected with a broad ridge which runs westwards towards the mountain promontory terminating in a peak marked β in Mädler's map. This depression is well worth examination. It seems to be due to a landslip or collapse of the border—a not unlikely occurrence in a neighbourhood which exhibits many indications of surface disturbance in the form of remarkable crumplings and foldings.

There is a distinct cleft running some little distance on the west of Macrobius. It is traceable from the foot of the north-east wall of Macrobius C to a point about midway between the peak β and the north-west wall of Macrobius, terminating on the south side of the broad ridge just mentioned (Schmidt alone shows it). It gradually widens as it proceeds northwards and ultimately assumes the character of a narrow deep valley. A row of 8 or more shallow depressions, which is not shown in the maps, figures on the outer slope of the west wall of Macrobius and follows its curvature. Schmidt draws a group of 5 or 6 minute hills in the central part of the dark floor. These appear to stand on a large circular mound of no great altitude, which is, however, prominent for some time after sunrise.

Macrobius C, the large ring-plain on the west, is separated from Macrobius by a valley of not more than 8 miles in breadth, through part of which runs the cleft. There is a wide break in the northern wall of this formation, partly occupied by two large craters, side by side, the more westerly being of the normal type and the companion very irregular in shape, with a narrow valley traversing its border and extending beyond its limits on the north. The west side of the interior is occupied by a low wide bank which impresses one with the notion that it resulted from the inflow of some semi-fluid material through the gap in the north wall. The floor of C is slightly lighter in tone than that of Macrobius, and has no central mountain or other details, except the mound flanking the western wall.

Beaumont House, Shakespeare Road,
Bedford, 1891, Sept. 19.

THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Madras Observations of Biela's Comet in 1805.

GENTLEMEN,—

The second observed return of this remarkable and now defunct periodical comet took place in 1805, when it was first seen by Pons at Marseilles. The probability that it was the same comet which had been discovered by Montaigue at Limoges, in 1772, was recognized in 1805, but it was then thought that the period was about thirty-three years, instead of a fifth part of that duration, as it was ascertained to be on the occasion of its return in 1826, when it obtained the name of Biela's comet.

It has been generally supposed that all the observations obtained at the appearance in 1805 were made in Europe. It approached the Earth on this occasion within the distance of less than five millions of miles, being nearest to us on December 9. So rapid was its southerly motion early in that month, that it would have been best seen in the southern hemisphere; but that half of the world did not yet possess any observatory, and was only visited by astronomers when some special purpose was in view. The East India Company had, however, for some years had an observatory at Madras (the same establishment which is now in want of a head, as it also was, by a remarkable coincidence, the last year that the British Association was presided over by an astronomer—Lord Wrottesley in 1860). Its Director, at the time to which we refer, was Mr. Goldingham, but the acting astronomer was Capt. Warren, who was himself also absent in the early part of December 1805. But Lieut. William Morison, of the Madras Artillery, was then at the Observatory, and has left a record of the appearance of the comet, with rough notes of its position by its places amongst the stars, in the constellations Grus and Toucana, on the nights of December 10 and 11. These are to be found in a manuscript volume of Madras observations (astronomical and meteorological, in the years 1804, 1805, and 1806), which, though evidently a fair copy, has never, I believe, been printed. It is now in the library of the Royal Astronomical Society, and I am obliged to Mr. D. Packer for calling my attention (in the absence of Mr. Wesley) to it.

Lieut. Morison heads his paper "Remarks on a Luminous Body observed on the 10th of December 1805, in the constellation of the Crane." He was much struck with its appearance, which he says "would have answered to the description of a Comet whose tail was turned towards or from the earth;" but he considered it unlikely that it was really a body of that nature, on account of the tremendous rapidity of its apparent motion in the sky. It was easily visible to the naked eye, being in fact, according to Lieut. Morison, comparable in brightness to a star of the fourth magnitude.

His description agrees with that of Olbers, who had observed the comet at Bremen the night before (December 9) and says ('Berliner Astronomisches Jahrbuch' for 1809, p. 136) that it was "sehr schön zu sehen, und blieb noch selbst nach dem Aufgange des Mondes dem blossen Auge sichtbar."

I cannot understand why these early Madras Observations have never been published, as they have every appearance of having been intended for publication. Perhaps it may be deserving of mention that, although no observation of this comet is recorded in the Greenwich Observations for that year, yet the volume of the 'Berliner Jahrbuch' just referred to does give an observation made at the Royal Observatory by Mr. Firmayer, of whom, I believe, nothing more is known; this observation is duly included in Carl's list in his well-known and useful "Repertorium der Cometen-Astronomie."

Yours faithfully,

Blackheath, 1891, Sept. 3. W. T. LYNN.

P.S. (Sept. 9).—Since writing the above, Dr. Dreyer has called my attention to a paper by Dr. Winnecke in the 15th Jahrgang (1880, p. 372) of the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' in which an account is given of some observations of Biela's Comet in 1805 made by MM. Malavois and Dupeloux in the Isle of France (Mauritius). Those of M. Malavois were made with a sextant on the 14th of December. The following is his description of the comet's appearance, which is very interesting when read in the light of its subsequent history:—

"Cette comète est photosphérique, c.-à-d. entourée d'une sphère lumineuse dont le diamètre m'a semblé être en totalité d'environ 45 minutes; mais la partie la plus lumineuse ou l'auréole vue au commencement du crépuscule du soir n'était guères que de 20 à 25 minutes; on la distinguoit alors très bien, tandis qu'à peine on pouvoit apercevoir les étoiles de 7$^{\text{me}}$ grandeur. La comète vue avec une lunette qui grossit seize fois le diamètre des objets m'a paru divisée en deux par une petite bande obscure; j'ai jugé son diamètre apparent d'environ une minute, mais les bords m'ont semblé confusément terminés et se fondre avec la lumière nébuleuse; une étoile de 4$^{\text{e}}$ à 5$^{\text{e}}$ grandeur que je distinguois très bien dans l'auréole à 4 ou 5 minutes de la comète, s'est trouvée dans une position et une distance différente par rapport à cet astre dont le mouvement propre était en effet considérable, comme on va la voir."

With regard to the expression that the comet appeared to M. Malavois to be divided into two by a small dark band, Dr. Winnecke asks whether we may find in this "einen Anfang der Vorgänge, die zu dem höchst merkwürdigen Umstande Anlass gaben, dass 1846 und 1852 statt eines Cometen deren zwei beobachtet wurden, und dass man später den Cometen überhaupt, als solchen wenigstens, nicht wieder geschen hat?"

M. Malavois states that from four days before his observation (*i, e.* on the 10th of December) MM. Laprie and Dabadie, the one censor, the other professor at the colonial Lyceum, had noticed the comet (" un astre ayant l'apparence d'une belle étoile nébuleuse "), and that it rapidly traversed, in a direction nearly from north to south, the space between the constellations Grus and Pavo.

W. T. L.

Sir William Lower's Observations of Halley's Comet in 1607.

GENTLEMEN,—

There seems to exist much misconception with regard to the observations of this comet in 1607 by Harriot and Lower, probably in great part due to the mistakes made by Baron von Zach in deciphering the manuscripts which he discovered in 1784 and published an account of them in the 'Berliner Astronomisches Jahrbuch' for 1788, besides other later papers respecting them in the 'Monatliche Correspondenz,' &c. Prof. Rigaud gives a corrected version of the observations in the Appendix to his classic 'Miscellaneous Works and Correspondence of the Rev. James Bradley.' That work appeared in 1832; but it is as well to mention that Rigaud published a Supplement to it in the following year, which is bound up with it in the R.A.S. copy, but is apparently not in the library of the British Museum.

In the life of Harriot in the 'Dictionary of National Biography' we read:—" He first saw the comet of 1607 (Halley's) from Ilfracombe on 17 Sept. His observations upon it were made with a 'cross-staff,' giving the distances of the nucleus from various stars."

Now it happens that the whole of Harriot's observations of this comet were made at Sion House, near Isleworth, where he was allowed a residence by his patron the Duke of Northumberland. (One of von Zach's numerous mistakes is that Harriot observed at Sion College, which was not built until several years after his death.) The first was on the 21st of September; the last on the 13th of October, when the tail was " obscurissima."

The observations of the same comet made off the coast of Devonshire and in Carmarthenshire were by Sir William Lower, who sent an account of them, in a letter dated Sept. 30, "to his especiall good friend Mr. Thomas Harriotte att Sion neere London." He first saw it, he says, at Illford combe (the old name of Ilfracombe); but it appears, by the later account in the letter, that "at" means "off" that place, after he had embarked in a ship for Wales, when about midnight he perceived the comet. Crossing over the Bristol Channel to Kidwelly (a place the mention of which von Zach overlooked and fancied the subsequent observations were made in Devonshire) he observed the comet again; and at a place called Traventi *, on Mount Martin, near that town (where he appears to have possessed an estate through his wife), he made observations, with a cross-staff sent down for the purpose of measuring land, from the 22nd of September to the 6th of October, including every night in the interval excepting four when the sky was cloudy and one when he "was a gossipinge."

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* In the Appendix to Rigaud's 'Bradley' this is called Irakenti; but in the Supplement we are told that the correct reading of the MS. is certainly Traventi.

Of Sir William Lower not much is known. I was led to these remarks by a letter of inquiry from Mr. Arthur Mec, F.R.A.S., of Llanelly, Carmarthenshire, who asked information respecting this astronomer who observed in that county. Finding, however, that he was really a Cornishman, I wrote to Mr. Dunkin, who tells me, in a letter dated Sept. 16:—

"The Lower family were settled chiefly in the parishes of Landulph and St. Winnow in Cornwall. The Sir William Lower who corresponded with Harriot is mentioned in the 'Bibliotheca Cornubiensis' and E. H. W. Dunkin's 'Monumental Brasses of Cornwall.' He was the eldest son of Thomas Lower of St. Winnow and Jane his wife, one of the four daughters and co-heirs of William Reskymer." Mr. Dunkin then quotes from his son's book:—"William Lower of Traventy, co. Carmarthen, returned M.P. for Bodmin on Sept. 27, 1601, knighted at Theobalds on May 7, 1603, and married to Penelope, daughter and heir of Sir Thomas Perrott, Kt., and niece of William, Lord Knollys, of Grays. He died intestate on April 12, 1615, and on May 17 following an administration grant was taken out by his widow, who afterwards married Sir Robert Naunton, Kt., Secretary of State to James I."

I may add that the Sir William Lower—who supported the royal cause in the early part of the Civil War and afterwards took refuge in Holland, where he wrote a number of dramatic poems which acquired little fame—was the nephew of the astronomer who corresponded with Harriot. He died in 1662.

Yours faithfully,

Blackheath, 1891, Sept. 18. W. T. LYNN.

Berne Time and the 'Nautical Almanac.'

GENTLEMEN,—

It is generally understood, I believe, that Dr. Hind will, before long, vacate the post of Superintendent of the 'Nautical Almanac.' Captain Noble's letter in your number of the present month seems therefore to be somewhat needlessly strong in its sneer at his shortcomings. But in the present case it is, I venture to think, undeserved.

Evidently some selection must be made of Observatories to be included in the National Ephemeris. Even so long a list as that in the 'Berliner Jahrbuch' might not include all of which some one might want to refer to, and I do not find from the list by Mr. Lancaster that the Observatory at Berne publishes any series of observations, and its work may well have escaped the notice of the

Superintendent. That its time is generally used in Switzerland is hardly a claim to be entered among working astronomical observatories, for astronomers generally give their dates in Local Time.

No doubt the list of observatories in the 'Nautical Almanac' wants, like many other things in it, revision; and that this may be satisfactory there should be a Committee or Board to receive and decide on suggestions. It is hardly fair to leave the Superintendent to bear the whole burthen however eminent he may be; and an official whose acts are commented on in the spirit of Capt. Noble's letter soon becomes, if he be not naturally, pachydermatous and ultra conservative, at all events it is to be hoped that he ceases to care.

Gentlemen who want the positions of the less known observatories would do well to get the lists published at Brussels from time to time by Mr. Lancaster.

Yours faithfully,
J. F. TENNANT.

11 Clifton Gardens,
Maida Hill, W.,
1891, Sept.

Berne Time.

GENTLEMEN,—

With reference to the above, I am in receipt of "Liste Générale des Observatoires et des Astronomes, des Sociétés et des Revues Astronomiques," by A. Lancaster, Brussels, which gives the longitude of Berne at $0^h\ 29^m\ 45^s{\cdot}66$ E.

Yours truly,
T. W. BACKHOUSE.

Jupiter's Satellites.

GENTLEMEN,—

Permit me to supply the following supplement to the Table which was printed at p. 321 of No. 167 of the 'Observatory,' subject to a correction noted at p. 392 of No. 169. The supplement will be especially useful in dealing with the case where a satellite is near its elongation. The satellites are supposed to move uniformly in circular orbits at their mean distances from the planet, in which case the times corresponding to the tabular distances as seen from the Sun are the same as for those seen from the Earth when Jupiter is in quadrature. If greater accuracy is required, the numbers in the table of any satellite should be altered in the proportion of half the mean synodic period to the 'Nautical Almanac' interval between successive conjunctions. Very great advantages may be expected to be derived from the method of measurement of the positions of Jupiter's satellites relatively to each other, now being adopted at Yale Observatory and perhaps elsewhere.

Faithfully yours,
A. FREEMAN.

Murston Rectory, Sittingbourne,
1891, Sept. 15.

Supplementary Table of the mean synodic motions of Jupiter's Satellites, from conjunction with the planet, as seen from the Sun. The distances are given in terms of the equatorial radius of Jupiter, and are measured parallel to his equator and perpendicular to the heliocentric radius.

First.				Second.			
r	h	m	s	r	h	m	s
5·0	6	46	35	5·933 ..	9	13	39
5·3	7	28	4	8·6	15	33	26
5·5	8	1	13	9·0	17	10	5
5·6	8	20	37	9·1	17	40	30
5·7	8	43	6	9·2	18	15	50
5·8	9	11	6	9·3	18	59	30
5·9	9	54	21	9·4	20	5	24
5·933 ..	10	37	9	9·439 ..	21	19	28

Third.				Fourth.			
5·933 ..	11	5	13	5·933 ..	14	27	27
9·439 ..	18	32	49	9·439 ..	23	19	7
14·5	35	31	44	15·057 ..	38	41	42
14·6	36	14	10	26·0	88	14	38
14·7	37	1	30	26·1	89	34	57
14·8	37	55	59	26·2	91	6	31
14·9	39	2	29	26·3	92	55	58
15·0	40	36	56	26·4	95	21	47
15·057 ..	42	59	54	26·486 ..	100	31	17

Total Solar Eclipse, April 16, 1893.

Gentlemen,—

The eclipse observers have certainly read with great interest Mr. Cruls' letter in the last number of the 'Observatory.' It appears from this letter that the weather is usually very bad in April on the coast of Brazil, and that therefore the chances of observing the total eclipse of 1893 from the province of Ceara are very small.

I have myself made inquiries as to the climate of Ceara during recent years, and am convinced that the prospect of observing the eclipse from Brazil is almost hopeless.

Mr. Boris, the French agent in Fortaleza, has kindly sent me a detailed list of the rainy days in Ceara during the last six years. The following is a *résumé* of this list, with the addition of Mr. Cruls' information:—

April	1885,	25	rainy days and	0^{m}·617	rainfall.
„	1886,	18	„	0 ·293	„
„	1887,	21	„	0 ·512	„
„	1888,	8	„	0 ·200	„
„	1889,	5	„	0 ·039	„
„	1890,	14	„	0 ·299	„
„	1891,	20	„	0 ·262	„

Mr. Boris adds that, although the month of April is the most rainy, it happens sometimes that after a severe rain the weather clears up and remains uncloudy for several days.

Fortaleza, the chief town of the province of Ceara, is within the zone of totality, but too far off from the central line to be a good station. However, it would be easy to get by sea to Para Curù, which lies nearly exactly on the line of totality and where there is an excellent port sheltered from the south-east winds. With a little more trouble it would be possible to set up the instruments on some high point in the interior, for instance in the little town of San Cruz or even on the Sierra do Machado, at 600 metres above the sea-level.

Following the line of totality across the Atlantic, we see that it meets the African coast between Dakar and Bathurst. A little factory, named Joal on the Admiralty chart, lies at about 20 kilometres from the central line, and would be a good station. It could easily be reached from Dakar by a special steamer. April is the best time of the year in these parts, and, contrary to what is to be expected in Brazil, the observers may count upon clear weather in Senegal. Not a single rainy day has been recorded here in April; and the heat is quite tolerable, the mean temperature being 20°·8 C. Moreover, the African coast can be reached in a few days, and the whole expedition need only occupy five weeks, including a clear fortnight for fitting up the instruments before the phenomenon.

It is true that the astronomical conditions are best in Brazil, the Sun's altitude being 75° and the totality lasting $4^m\ 42^s$. But on the west coast of Africa the difference is slighter—the Sun is 54° high and the duration is $4^m\ 13^s$. The loss of 29 seconds of totality is amply counteracted by the chances of clear weather.

I think, therefore, that European astronomers would be wise in selecting their stations on the coast of Senegal, where they will find, from the French government, all the help they require.

It is to be hoped that the long duration of totality will be advantageously employed and that astronomers will make the most of this eclipse, as it will not be until 1896 that another opportunity of observing such a phenomenon will occur.

If different nations intend to send out expeditions in 1893, it would be desirable that there should be a general agreement between the observers in order to avoid duplicate work being done. In recent eclipses many observers worked exactly on the same lines, thus wasting the precious moments of totality.

An international convention would prove useful in furnishing a general discussion of the results obtained in the last expeditions and in preparing new plans of action.

The Solar Physics Committee seems to be the most efficient agency for taking in hand the initiative of such a kind of international congress. Yours truly,

Paris, 1891, Sept. 18. CTE. DE LA BAUME PLUVINEL.

[It is to be hoped that by the time of this eclipse the political condition of Chili will be so far settled as to allow the Director of the Observatory at Santiago to make good use of his opportunities. The central line passes only 200 miles N. of his observatory; and the wide interval separating him from the probable location of other observers, as indicated by the interesting letter of our correspondent, suggests that the present is again a suitable opportunity for detecting possible changes in the corona. We fear that the idea of an international conference is not very practicable; but meanwhile we would remind our readers that the Eclipse Committee of the Royal Astronomical Society was constituted with the avowed object of being a centre of information to intending observers.—Eds.]

OBSERVATORIES.

The following notes on the work of some foreign observatories during the year 1890 have been abridged from the 'Vierteljahrschrift der Astronomischen Gesellschaft,' similarly to those in Nos. 166, 154, 126, &c.

Basel. *Dr. Riggenbach.*—This observatory is for the use of students in the University. No regular work is done.

Berlin. *W. Foerster.*—No important change has occurred in the staff or instrumental equipment. Dr. Küstner obtained with the large transit 3454 observations in R.A., and 3070 in Dec. Besides working off the Pulkowa list of stars, a large inroad has been made in a list of some 600 stars, for Dr. Auwers, in zone +15° to +20°. The proximity of heavy machinery appears to have an increasing effect on the observations made with the large transit. The stars have a rapid oscillation, and appear double with an apparent distance of some 2″ or 3″. That this is due to a trembling of the instrument as a whole is shown by the vibration of the circle-division under the microscope. The disturbance may possibly arise from the new plant at the works of Messrs. Siemens and Halske some 200 metres distant.

Dr. Knorre, with the 9-inch equatorial, has determined some 700 star-places, has made 310 estimates of magnitude, and has obtained a few observations of comets, small planets, and double stars. All the reductions are in a satisfactory state. The time-service has been carried on as usual.

Bonn. *Deichmüller.*—Dr. Schönfeld, the former Director, died on May 1. The principal work of the observatory has been the completion of the zone +40° to +50°, which has been in hand since 1878. The first 22 catalogue-sheets (0^h to 21^h) contain 16,011 stars, each of which has been observed 2 or 3 times. Various astronomical calculations. Meteorological observations.

Breslau. *J. G. Galle.*—Usual routine work, chiefly meteorological.

BRUSSELS. *F. Folie.*—A combination of particularly unfavourable circumstances has influenced the progress of astronomical work. One assistant has resigned, and another has had to be transferred to the physical department in consequence of weak health; the sky has been clouded for the greater part of the year; and finally there has been the removal of the observatory to Uccle. The meridian work has suffered most, but the transit is now in working order. M. Niesten with the 38-centimetre equatorial has prosecuted his observations on the physical aspect of Mercury, Venus, Mars, and Jupiter. He has also regularly observed the phenomena of Jupiter's satellites, and occultations of stars by the Moon, besides making a few double-star measures.

Since the death of M. Fiévez the spectroscope has been in charge of M. Spee.

LIÈGE. *M. Folie.*—This observatory is attached to the Brussels observatory, and the observations are published in the Brussels volume. At present the meridian circle is dismounted and is with Messrs. T. Cooke and Sons. Attention seems to be given to observations of stars near the Pole with the transit, and of comets, small planets, and variable stars with the equatorial.

DRESDEN. *Dr. B. von Engelhardt.*—12-in. Grubb equatorial. Observations of Comets Brooks 1889 V.; Brooks 1890 II.; Coggia 1890; Denning 1890 VI.; Zona 1890 IV.; Spitaler 1890 V.

DÜSSELDORF. *R. Luther.*—With the 7-inch, 29 observations of 15 small planets were made. Calculations of ephemerides.

GENÈVE. *R. Gautier.*—The 10-inch equatorial has been utilized in making 23 observations of Comet Brooks 1890 II., 2 of Zona 1890 IV., and 1 of Denning 1890 VI. A small equatorial has been used by M. Emile Gautier for spectroscopic work on the Sun. Meteorological work. 1520 pocket-chronometers and watches have passed the observatory in 1890. This is treble the number in any previous year.

GOTTINGEN. *W. Schur.*—The 6-inch heliometer has been in regular use for two years. Observations of Victoria, Sappho, and comparison stars for Dr. Gill. Triangulation of Præsepe completed. Twelve observations of Comet Brooks 1890 II. Some double-star measures. Parallax observations of 70 Ophiuchi.

GOTHA. *Harzer.*—Observations for proper motion. Published in the 'Astronomische Nachrichten.'

GRIGNON. *M. Lamey.*—With respect to the drawings made of Mars, M. Lamey remarks that there are enormous differences between his own and those made by his assistant M. Démoulin; but each observer's pictures agree *inter se.* A considerable number of drawings of Jupiter, and 9 of Saturn.

HAMBURG. *Rumker.*—With the transit a number of observations were made of comparison stars for comets and small planets. Equatorial observations of comets and the fainter asteroids. Chronometer work for the German navy. Time-service.

Heidelberg. *Max Wolf.*—Possesses a 162-mm. equatorial and a large number of smaller instruments and photographic objectives. Researches on errors of photographic objectives. Photometric photographic researches on star-clusters. Coloured stars. Drawings of lunar craters. Time-service.

Herény. *Eugen v. Gothard.*—Influenza unfortunately occupied the Astronomer's time. 48 photographs were taken during the solar eclipse of June 16.

Jena. *Knapf.*—This observatory was founded in 1812 by the Grand Duke of Saxe-Weimar. Observations of comets, occultation of stars by the Moon, phenomena of Jupiter's satellites. It is also a meteorological station.

Kalocsa.—*J. Fényi, S. J.*—Solar work. The Sun was observed on 174 days, on 67 of which it was free from spots.

Kiel. *Krueger.*—"Centralstelle für Astronomische Telegramme." Steinheil refractor 216 mm. Observations of newly discovered comets and small planets. Meteorological work. Computation of ephemerides.

Königsberg. *C. W. F. Peters.*—Meridian observations of stars 83° and 84° north dec., of comparison stars, &c. The heliometer was used for double-star measures; Comets Brooks 1890 II. and Zona 1890 IV., and observations for parallax. Lunar work. Meteorology.

Kremsmünster. *Col. Wagner.*—Ring-micrometer on a 6-inch equatorial. Comets Brooks 1890 II., Coggia 1890 III., Denning 1890 VI., and Zona 1890 IV. Nebulæ. The Sun was observed on 220 days, on 147 of which there were no spots. Magnetic and meteorological work.

Leipzig. *H. Bruns.*—The observations of Victoria, Sappho, and comparison stars have been reduced, and the results forwarded to Dr. Auwers. Zone work + 5° to + 10°, and also + 10° to + 15°. Parallax of stars having large proper motions—Arg. Oeltz. 10603; β Comæ; Lalande 27298; Br. 3077; μ Cassiopeiæ; and 31 Aquilæ. Triangulation of trapezium in Orion nebula.

Lund. *Möller.*—Of the stars in the zone + 35° to + 40° some 24334 observations have been made, and of these 21136 have been reduced to mean equinox 1875·0. The publication, interrupted by the transfer of Dr. Dunér to Upsala, will now go on, so that the third volume will shortly be out.

Milan. *Schiaparelli.*—575 measures of double stars were made with the 18-inch refractor. 219 with the 9-inch. Longitude work. Meteorology. Time-service.

Munich. *Seeliger.*—$10\frac{1}{2}$-inch refractor. Micrometric measures of Saturn's system; observations of comparison stars for Comet 1889 V.; a few double-star measures; Comets Borelly 1890 I.; Brooks 1890 II.; Denning 1890; d'Arrest 1890; Zona 1890 IV. These are reduced and published in the Ast. Nach. Differential measures of the cluster 384 Persei will appear shortly. Longitude work.

O'GYALLA (UNGARN). *Konkoly.*—The principal work of the observatory is on the solar surface. Of the 205 days when observations were made, the Sun was free from spots on 128 days. Spectroscopic work. Observation of meteors in conjunction with Pressburg.

POTSDAM. *Vogel.*—Spectroscopic work on close doubles, &c. Solar photographs. On 125 days out of 281 the Sun was free from spots. Stellar photography. Photometric work. Meteorology.

PRAGUE (University). *Weinek.*—Lunar drawings with a 6-inch refractor and lunar work generally. Estimations of altitude in conjunction with Berlin and Potsdam. Phenomena of Jupiter's satellites. Time-service. Magnetic and meteorological work.

PRAGUE. *Safarik.*—Work on Mars, Jupiter, Uranus, and Neptune. Prof. Safarik made 1461 estimations of brightness of 123 variable stars.

STRASSBURG. *E. Becker.*—Observations of Comets 1890 II., III., IV., V., VI., and comparison stars. Meridian observations of the Sun, Moon, Mercury, Venus, and zone-stars. A series of altitude-estimations with pairs of stars on 23 evenings.

UPSALA. *Dr. Dunér.*—Variable stars. Experimental work in stellar photography.

ZURICH. *R. Wolff.*—Sun-spot observation. 298 observing days and the Sun was free from spots on 150 of these.

COLUMBIA (MISSOURI). *Updegraff.*—The director's time is largely occupied in the mathematical and astronomical classes of the University. There is a small transit, a 7½-inch refractor, a spectroscope, and a 2½-inch altazimuth. Mr. Updegraff hopes soon to get into some regular work.

HAVERFORD COLLEGE. *Leavenworth.*—Work on stellar parallax. Already 143 observations of Σ 634, 2777, 2877, 3008, 3127, and South 403 have been made. Sun-spot observations on 266 days.

BIRR CASTLE.—Lord Rosse's assistant, Dr. Otto Boeddicker, has published* the observations made during the total lunar eclipse of 1888, January 28, on Lunar Radiant Heat. There is an interesting introduction by Lord Rosse, of which we venture to reproduce the substance.

"Some years ago it was suggested that it would be desirable to turn the large Reflectors at Parsonstown, armed with the thermopile, upon the Moon, with the view of detecting, and, if possible, measuring its radiant heat. Several attempts to do so had been made elsewhere, but without encouraging results. Melloni had employed a lens of three feet aperture of imperfect quality, but sufficiently good for the purpose. Owing, however, to the large absorption of heat by glass he obtained no very certain indications; his pile could not have received any of the less refrangible heat-rays through the glass, but it appeared quite possible that with a speculum of equal aperture far more decided and really interesting and instructive results might be arrived at."

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* Trans. Roy. Dubl. Soc. n. s. vol. iv. part ix.

After some early experiments, two thermopiles, with concave reflectors, were placed in the focus of the three-foot telescope, and the Moon's image turned alternately upon each pile.

"The needle was at all times more or less, and often very unsteady, though by taking means of a long series of alternate readings it was found possible, not only to demonstrate the existence of a very appreciable amount of radiation, but even roughly to estimate its variation with the phase and form, a curve generally representing that variation. The results were published in the *Proceedings* of the Royal Society, No. 112, 1869."

As the next step Lord Rosse was "able by the interposition of a sheet of glass to show that the Moon's heat differed materially from the Sun's in that it contained a much larger proportion of rays of low refrangibility. From the very rapid diminution of the heat towards New Moon it was probable that at that phase the remaining heat would be scarcely, if at all, appreciable; and from these two facts I concluded that the heat which I had been attempting to measure was heat received by the Moon from the Sun, but only re-radiated after part of it had been absorbed by the lunar surface, and then emitted as dark rays of heat. At the same time, so far as could be gathered from these rough determinations the maximum of heat did not take place as might, perhaps, have been expected to an appreciable degree after the maximum of light, that is after Full Moon. Some rough determinations of the proportion between lunar and solar radiation were also made."

Dr. Copeland, on being appointed assistant at Birr Castle, carried on and extended these researches and found that "from some unexplained cause the maximum of heat seemed to occur somewhat before Full Moon."

"The striking result having been arrived at that the maximum of heat did not lag behind Full Moon, it appeared desirable to proceed a step further, and as a more rigid test to try how far the minimum of heat, which presumably existed during the progress of a lunar eclipse, coincided with, or came later than, the middle of that eclipse."

The first results of importance were obtained on 1884, October 4. "Dr. Boeddicker was on the spot with the apparatus in position from the commencement of the penumbra until forty minutes after the last contact with the penumbra. Clouds intervened until twenty-three minutes before totality, when the sky became perfectly clear, and remained so until the end of the observations. The heat as before diminished, and increased again nearly proportionally to the light, becoming inappreciable on reaching the limits of totality. The minimum of heat apparently fell later than *that* of illumination. But the most remarkable thing was that while during the short interval between the first contact with the penumbra and the commencement of total phase all appreciable radiation vanished, between the end of total phase and the last contact with the penumbra, and even forty minutes later, the heat had not returned to the standard for Full Moon, being deficient by about twelve per cent. This we failed to explain, where we might expect to do so, by any derangement of the apparatus, nor could we trace it to any physical cause. The above observations were published in these *Transactions* for 1885 (vol. iii., series 2).

"The next opportunity, an exceptionally favourable one, occurred on January 28, 1888. The sky was not obscured at all during the whole progress of the eclipse, and the same anomaly of the heat not returning to its standard value, even 1 hour 40 minutes after the last contact with the penumbra, was observed. Also the radiation which we began to measure 1 hour 5 minutes before the first contact with the penumbra seemed to begin to decrease almost immediately; and even if the first observation be excluded, yet the decrease of heat seemed to begin as early as 15 minutes before the commencement of the eclipse. These results would appear to be particularly interesting as suggesting a terrestrial atmosphere much more extensive than it has generally been supposed to be which intercepts the Sun's rays of heat long before any part of the Moon has reached the Earth's shadow. We would much desire another opportunity for attacking the subject. As, however, it is exceedingly unlikely that the possibility of measuring the heat during an eclipse under similarly favourable meteorological conditions will soon recur here, and as it does not seem profitable to spend more time in getting a slightly more accurate phase-curve, it seems desirable that the present communication should close the series. If we pursue the subject of radiant heat further, it will be by making a new departure with apparatus modified so as, if possible, to combine greater sensibility with greater freedom from extraneous influences, varying also the methods, and extending the subjects and objects of investigation.

"Dr. Boeddicker has devoted a great amount of time and labour to the fullest reductions, and to the formation of the theoretical curve for the Moon's light, possibly more than was justified by the probable errors of the observations; but as the occasion was quite an exceptional one, it appeared best to leave nothing undone to get the utmost out of the night's work."

PUBLICATIONS.

The Draper Catalogue of Stellar Spectra.—Volume xxvii of the Harvard Annals is a new departure in astronomical literature. It is a catalogue of the photographic spectra of more than ten thousand stars. Hitherto catalogues have been made of the positions or magnitudes of stars, or of the positions and geometrical characteristics of nebulæ: but a general index to the physical nature of ten thousand objects is a novelty of the first importance and cannot well fail of its avowed object, "to perpetuate the name of Dr. Draper by a research in the branch of science to which he devoted the last years of his life."

In explanation of the actual accomplishment of what a few years back would hardly have been dreamed of, Prof. Pickering points to two causes as having greatly increased the number of stars whose spectra can now be studied photographically. The first is the invention of the dry plate; and it will take us a long time to realize all that we owe to the inventor. The second will appear to many of a curiously different order of importance—"the substitution, for a slit spectroscope, of a prism placed over the objective, thus saving a great portion of the light." Perhaps, however, it is our present familiarity with this method which causes surprise that it should be ranked as a distinct and recent step in advance; and Prof. Pickering doubtless has good reason for the remark.

The photographs were taken with an 8-inch Voigtländer lens, before which was fastened a prism 8 in. square with a refracting angle of 13°. The edge was perpendicular to the Earth's axis, so as to disperse the light in declination, the length of the spectrum being about a centimetre; and the star being allowed to trail slightly gave the requisite width of a millimetre to the spectrum. Each plate covered 10° square, and the spectra of all stars to the sixth magnitude were photographed. These were divided into classes, much more numerous than those in ordinary use on any other system, and lettered accordingly. The letters A, B, C, D indicate varieties of the first type; E to L varieties of the second type; M the third type; N the fourth type; and O, P, Q, spectra which do not resemble any of the preceding types. All forms of spectra except A, E, and H require further explanation, which is accordingly given in the remarks following Table II.

One of the most important features of the work is the method by which photographic magnitudes have been assigned. "The quantity measured in each case is the intensity of the spectrum in the vicinity of the G line. Accordingly, when stars having different spectra are compared, the results will not be the same as if the entire light of the stars were measured. In the latter case, the results will differ with the colour of the star according to the method of measurement employed. This is a serious defect in the measures of the brightness of the stars in catalogues hitherto published. Since the present measures relate to rays of a single wave-length the same result should be obtained whether the method of comparison was by the photographic plate, the eye, or the thermopile."

Coming to the Catalogue itself we may say briefly that it contains in a compact form the approximate positions for 1900 of 10,351 stars, with their reference numbers in the Bonn Durchmusterung and the Harvard Photometry: their class of spectrum as above; their photographic magnitudes and the differences of these from the magnitudes of the D.M., the Arg. Gen. Cat., and the H.P.

It is followed by another well-arranged table giving the details of the measures of magnitude on the various plates on which each star appears; for the whole heavens to −25° was photographed twice, with plates overlapping in the manner adopted for the astrophotographic chart, corner on centre; and several exposures were often taken on the same plate. To this again is added an appendix of copious notes and remarks.

It would be difficult to say too much in praise of the zeal and skill with which this great work has been accomplished. The name of Mrs. Fleming is already well known to the world as that of a brilliant discoverer; but the present volume shows that she can do real hard work as well. The bulk of the work of examining plates and preparing the copy for press was in her charge, though it was actually commenced by Miss N. A. Farrar. She was apparently aided throughout by ladies. The photography was in the hands of Messrs Clayton, Gerrish, and Gifford. To all these we owe a great debt of gratitude.

NOTES.

THE ALLEGED VARIATION OF LATITUDE.—The observations undertaken in concert at Berlin, Potsdam, and Prague for investigation of the fluctuations of apparent latitude have now been continued for more than two years at the first and third stations. Herr Th. Albrecht communicates the results to 'Ast. Nach.' No. 3055, and a very fine series of results they are, showing immense care and skill on the part of the observers.

The existence of a fluctuation of about 0″·5 in amplitude and period one year seems to be definitely established by the beautifully accordant observations, though the ultimate cause has yet to be assigned. Smoothed curves give the following figures :—

	Berlin.		Potsdam.		Prague.	
		52° 30′		52° 22′		50° 5′
Max. 1889...	Sept. 6	17″·55	July 23	56″·49	Aug. 1	16″·02
Min. 1890...	Feb. 16	17 ·05	Feb. 4	55 ·97	Feb. 16	15 ·53
Max. 1890...	Sept. 2	17 ·53	....		Sept. 9	16 ·07
Min. 1891...	Mar. 16	17 ·03	....		Feb. 27	15 ·65

THE ASTRONOMISCHE GESELLSCHAFT held its fourteenth biennial meeting at Munich, August 5–7, under the presidency of M. Gyldén. The Society numbers 318 members in all, 32 of whom are new. On August 6, after formal votes of thanks had been passed to the Bavarian Government for its hospitality; and tributes paid to the memory of deceased Fellows, M. Seeliger read the Secretary's report. M. Krueger was cordially thanked for the care with which he edits the 'Astronomische Nachrichten.' Papers were read by M. Bruns, of Leipzig, "On a new method of determining the division errors of the Transit-circle," and by M. Weiss, of Vienna, "On Cometary Orbits." The latter referred to 91 orbits calculated by Dr. Kleift, and suggested a central bureau for calculating such orbits. M. Peters presented some photographs of the total solar eclipse of 1851 July 28. Next day Utrecht was chosen for the Meeting in 1893. M. Bakhuyzen reported on the work of the observatories participating in the zone work. M. Franz, of Königsberg, undertook to determine the figure of the Moon and the coordinates of the principal craters from the Lick photographs. The members of a Bureau for calculating the orbits of small planets were nominated. There was a discussion on the variation of latitude.

The Gesellschaft visited the Bogenhausen Observatory and dined with M. Steinheil, when M. Palisa's last new planet was christened *Bavaria*, and M. Foerster announced the formation of a new Society at Berlin for studying meteors and terrestrial physics.

The Rev. M. H. Close very cleverly pulls to pieces, in a paper to the R. Ir. Ac., 1891 June 8, the elementary deduction of the variation inequalities in the Moon's motion and the elementary proof that the shortest axis of the variation orbit is directed towards the Sun, hitherto given in most text-books.

This elementary proof was first given by Newton himself, who, however, also gave a complete geometrical proof. It has thus escaped very careful scrutiny; and it has apparently been reserved for Mr. Close to show that, though the result comes out right, the process is faulty. An occurrence of this kind, in an almost purely mathematical region, is certainly a curiosity. We are familiar with working hypotheses which satisfy most conditions except that of being correct in the domain of Physics; but here is a mathematical train of reasoning in the same plight!

THE APPARENT POSITION OF SOLAR PROMINENCES.—Referring to the remarkable phenomena recently observed by MM. Trouvelot, Deslandres, Fényi, and others on the Sun's surface, M. Fizeau points out ('Comptes Rendus,' cxiii. no. 10, p. 353) that the enormous velocities with which solar prominences are endowed, according to the testimony of the spectroscope, should give them an apparent displacement, with reference to the Sun's limb, of the nature of an aberration. This aberrational effect may explain many observations recorded of prominences at some distance from the limb or within the limb.

We do not remember to have seen this remark before. It is certainly one of great importance.

The friends of the late Mr. N. R. Pogson are raising a fund for a memorial to him in Madras. They propose (1) that a brass plate with a suitable inscription should be placed in the Cathedral; (2) that a sum of money should be put at Mrs. Pogson's disposal for the education of her youngest son, a boy of about six, most of the money subscribed being devoted to this latter object. Subscriptions may be sent to Mr. C. Michie Smith, Government Observatory, Nungumbaukum.

We learn that the operations in connection with the proposed observatory on Mont Blanc have been suspended, and no more work is contemplated this year. A tunnel large enough for one man to pass along has been bored horizontally from north to south a distance of 23 metres, at a vertical distance of 12 metres from the summit. The tunnel is now closed.

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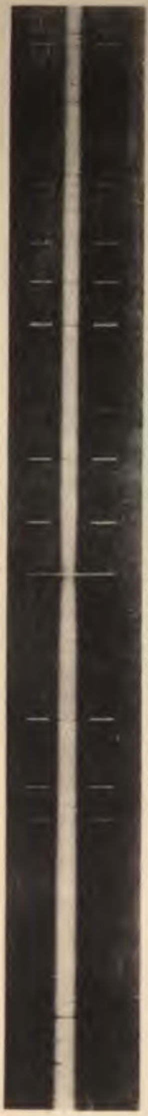
FIG. 1.



Spectrum of Sirius with comparison Spectrum of Iron.

FIG. 2.
α Aurigæ.

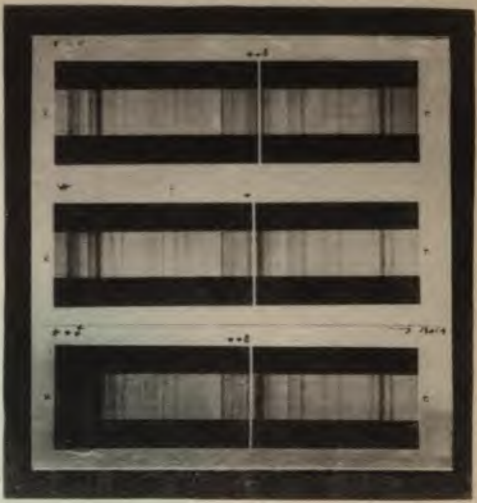
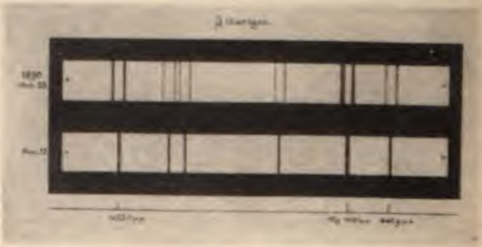


FIG. 3.



THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 181. NOVEMBER. 1891.

Outburst of Spots in the Northern Hemisphere of Jupiter.—On a recent Photograph of Jupiter obtained at the Lick Observatory.

In his interesting "Notes on Jupiter" in the October number of the 'Observatory' Mr. W. F. Denning refers to the great outburst of dark spots which has occurred lately upon the dark belt situated in about latitude 25° in the northern hemisphere of the planet. This outburst has been watched here as closely as circumstances permitted, and various interesting peculiarities have been noted. The spots are very numerous, over 20 having been observed and followed for considerable periods of time, and some of them move considerably more swiftly than others. It is premature at present to attempt any complete discussion of the results, which promise, however, to be highly interesting and instructive. But a change has recently taken place in the rate of motion of some of these spots of so remarkable and radical a character that a few words on the subject may not be out of place.

Mr. Denning has given some observations of one of the chief spots of the outbreak in the paper already referred to (see 'Observatory,' p. 330), and from these observations, extending from the 21st August to the 15th September, he derives $9^h\ 49^m\ 27^s.2$ as the period of rotation. The motion of the spot during this time was quite uniform. But a great change occurred about the 15th September. The motion became suddenly less swift, so that from thenceforth up to the 15th October the period of rotation has been $9^h\ 49^m\ 44^s.2$; 17^s longer than it was previously to the former date. A similar change also occurred in the case of other neighbouring spots, and at the same time. So that the period of rotation of the surface-material of Jupiter quite suddenly increased 17^s or so in duration over a wide region (in longitude). A change in rotational rate of this kind of such suddenness, extent, and magnitude is probably unique in the history of the planet.

The following are some observed times of transit of the spot particularly referred to by Mr. Denning, which will serve to supplement the list already given by him. The times marked with an asterisk have been kindly communicated to me by Mr. Denning:—

	h	m		h	m
1891, Sept. 22..	10	50·5	1891, Oct. 3..	12	17
„ ..	10	55*	4..	7	55·2
„ 27..	8	48·2	15..	9	18·2
Oct. 3..	12	17*	17..	10	31·9

Observation has been much hindered lately by the bad weather, and in view of the large number of spots and their different motions, it is very desirable that the times of the passages over the central meridian of the disk should be recorded as often as possible.

Observers of Jupiter have doubtless not infrequently wished that they could review the planet. Some region may not have been observed as closely as subsequent events may show to have been desirable. How nice, for instance, would it not be to be able to review the region known to have been occupied by the red spot in 1877, or some earlier year! The present outbreak of spots above referred to is another case, as in my earlier observations I did not pay that close attention to this region of the planet which I should have done had I known what was going to occur. But here circumstances have been more kind. I am indebted to the great kindness of Professor E. S. Holden for a positive copy on glass of a series of photographs of Jupiter taken by him with the great Lick telescope on the 19th August last. These photographs are of such a degree of excellence that an examination of them *is almost like looking at the planet itself*! It so happens that I had observed and drawn the region of the planet represented in these photographs on the same night, but about 10^{h} or one rotation earlier. In my drawing only three spots appear on the belt in about 25° N. lat. The Lick photographs show *six* distinctly, with traces of a seventh, and also indicate various interesting details not represented in the sketch. The markings on the S. side of this belt move so much more swiftly than those in other parts of the planet, that there is a marked displacement in the positions of the spots in the photographs relative to the red spot, though the photographs were only taken about 10 hours later than the sketch.

Various other interesting features are also well represented. On p. 330 of the 'Observatory' Mr. Denning gives some observations of a bright spot situated to the south of the red spot, and figures it in his sketch on the preceding page. This spot is quite a glaringly conspicuous object on the photographs. The red spot itself is, of course, exceedingly well shown, its interior being slightly lighter, so that it still has an annular form. Altogether there are 16 or 17 spots indicated with sufficient distinctness to enable their positions to be measured. But perhaps the chief attraction attaches to the 3rd satellite and its shadow. In the plate sent by Prof. Holden there are three images of Jupiter, respectively taken at 13^h 47^m 6^s, 13^h 50^m 46^s, and 13^h 53^m 16^s P.s.t. *. In the first of these the satellite appears as a clear round spot beginning to enter upon the planet's disk; in the second it is shown about half-on; whilst in the third it is almost entirely on the disk, and still perfectly obvious, though beginning to be lost in the brighter surface nearer the centre. In all three images the shadow of the satellite appears as a very prominent blackish spot covering the following end of the red spot, and having an obvious penumbral fringe †.

In the September number of the 'Observatory' Mr. Denning states that on the 21st August last the spot on the belt N. of the N. equatorial belt was "quite as obvious as the shadow of a satellite, but perhaps a little smaller." The admirable photographs of Prof. Holden show that two days earlier this spot was a mere trace compared with the great dark shadow of satellite III.

Burgess Hill, 1891, Oct. 21. A. STANLEY WILLIAMS.

The Large Sun-spot Group of Aug. 28–*Oct.* 4, 1891.

THE following notes on some telescopic and spectroscopic phenomena observed in this large group of spots are intended to supplement the note by Father Sidgreaves in the last number of the 'Observatory.' The group was born on Aug. 28 at the eastern limb, its mean heliographic coordinates being lat. 18°·5 N. and long. 221°·0. It died on Oct. 4, on the western limb, in lat. 22°·0 N. and long. 226°·7. It thus both formed and died on the visible hemisphere. Moreover it was the largest group of spots observed since June 1885, and one of the largest since the great November spot of 1882. Fourteen drawings were obtained of the group at Stonyhurst. During its first passage over the disk, Aug. 28–Sept. 10, it consisted mainly of two very large irregular spots. The following of these was by far the largest spot of the group, and underwent the greatest internal changes. It did not, however, survive to a second rotation; for then the preceding spot alone appeared, being accompanied in its passage across the disk by occasional outbreaks of small spots scattered over a wide area.

At its birth the group was surrounded by bright compact faculæ, one scimitar-shaped jet lying S. of the preceding spot being es-

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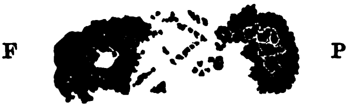
* These are the times of the ends of the exposures, which were in all three cases for 10^s.

† The shadow of this satellite seems to have been photographed for the first time by Dr. Common in 1880 with his 3-feet reflector. See 'Monthly Notices,' Vol. xli. p. 205.

Fig. 1.



Spot B. Spot A.

pecially remarkable. At its death the faculæ were still very bright, but were vastly more extensive.

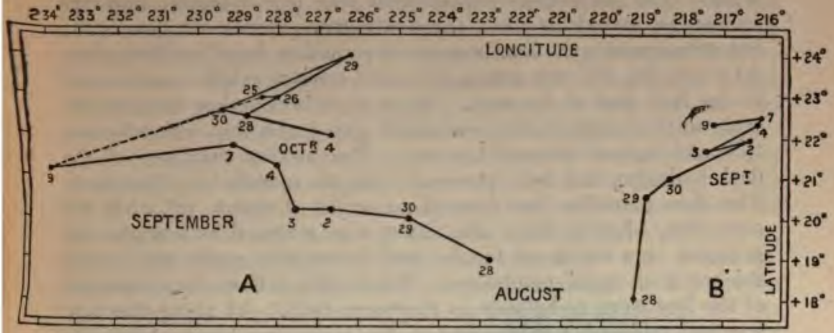
The area of the group at its birth on Aug. 28 was 77 millionths of the visible hemisphere. As it crossed the disk it steadily developed, and attained its greatest area of 1834 millionths on Sept 7. At its reappearance after one rotation on Sept. 25, the the area had diminished to 545 millionths. The diminution continued until the extinction of the spot on Oct. 4.

The group displayed some remarkable proper motions, the two members repelling one another, except for a slight approach on Sept. 3. At their birth on Aug. 28 the two spots were 3°·6 apart in longitude, which was increased to 5°·9 on Aug. 29th; to 10°·4 on Sept. 2; to 11°·8 on Sept. 4; and to 13°·0 on Sept. 7. Between Sept. 7th and the 9th, the following spot changed its retrograde motion to one of approach, but its companion gave at the same time such a leap forward, that on the 9th their distance apart reached its maximum of 16°·5. This forward motion of the whole group took place just after it had attained its greatest area.

Considering the motions of the individual spots, that of the preceding member of the group was very rapid, as it advanced from long. 222°·8 to 233°·8 between Aug. 28th and Sept. 9, or 11° in 12 days. Its most rapid drift of 4°·7 in two days took place between Sept. 7 and 9. The drift of the following spot in the contrary direction was not at all great; but 3°·1 in the 10 days, Aug. 28–Sept. 7, and from long. 219°·2 to 216°·1. Both the spots rose in latitude, the mean drift of the group being from 18°·5 N. to 22°·2, while the individual drifts were from 19° to 21°·8 and from 18°·0 to 22°·5 for the preceding and following spots respectively. Their common advance in longitude Sept. 7–9 was accompanied by a slight fall in latitude, so that their paths were almost parallel. The end of the first rotation was marked by the metallic prominence of Sept. 10 immediately over the following spot, which was then exactly on the limb.

At the beginning of the second rotation the preceding spot, which now alone remained, was found on Sept. 25 in lat. 23° and long. 228°·3. Excluding possible oscillations, it had therefore, when on the invisible hemisphere, drifted 1°·8 higher in latitude, but retrograded 5°·5 in longitude. Until Sept. 28 it remained almost stationary, but from the 28th to the 29th it suddenly rose 1°·5 in latitude and advanced 2°·6 in longitude. Next day it as suddenly

Fig. 2.



dropped to almost exactly its original position in latitude, and retrograded 3°·1 in longitude. This seems to have been its expiring effort, as on Oct. 4th it was represented by a group of minute scattered dots. Therefore while the following and larger spot remained comparatively stationary, the preceding spot drifted rapidly forward in longitude in the first rotation, and retrograded in the second rotation, so that when it died it was but 3°·9 in advance of its first position.

In the spectroscope the most important spot of the group was the one marked B in the figure. The following are some of the details of the variations in the hydrogen C line over the spots when they were on the visible disk. The observations were taken during a vacation visit to the Stonyhurst Observatory, at the kind invitation of the Director. On Aug. 30th, at 11 A.M. G.M.T., in a hazy sky, and employing a dispersion of 6 prisms of 60°, the line was seen to be reversed and displaced towards the violet in the preceding portion of the nucleus of the following spot. There was also a less brilliant reversal of the line noted in the faculæ which was situated between the two spots, while in that which followed the group the line was broken though unreversed.

The next observation was on Sept. 3rd in excellent definition, so that the full battery of 12 prisms of 60° and a high magnifying-power were employed. The following of the two spots was seen to be broken up into a number of separate nuclei, with penumbral and faculous matter filling up the spaces between. As on Aug. 30th, so now too, the C line was unaffected by the preceding spot. But in its companion some remarkable reversals and displacements of the line were observed. By carefully bringing the slit over various portions of the spot, it was possible to locate the chief seat of disturbance as being situated in the northern half of the spot, and in the faculous and penumbral gap which intervened between this and the southern half. At 11.30 the line was broken in the penumbral gap, and a small portion was twisted and blown towards the violet. It was pear-shaped, with the stem adhering to the dark line. It gave the impression of fluttering about, as if blown by a wind. The estimated displacement was 0·7 X-metre, and the appearance lasted for about three minutes. It was not seen again, but subsequently the line became very much widened on both sides. At 11.53 the line was reversed in that nucleus which was situated in the N.F part of the spot. At 12.15 it became less dark in this same nucleus, and in the penumbral gap had a thin hair adhering to it and turned towards the red. The motion therefore of the dark hydrogen had been reversed from an uprush to a downrush. The thin hair-like line formed an angle of about 30° with the main line. Lower down this latter was reversed as a bright dot probably in a region of faculæ, and below this again was greatly thickened in some penumbræ. Hence five different appearances of the line were to be seen in the same field. At 12.35 the thin line had vanished, a thickening of the main line having taken its place. When the group was sketched next morning at 9.45, it was seen that the aspect of the spot had changed in that very portion in which the disturbance had been observed in the spectroscope. On Sept. 6th, at 3 P.M., the C line over this same spot was less dark but otherwise unaffected.

On Sept. 28th, at 12 P.M., the C line was reversed in the nucleus of the preceding spot, which now alone survived. It is noteworthy that as in the case of its companion, so now too, a reversal of the hydrogen line heralded its speedy dissolution. Nor must we omit to call attention to the fact that the independent observations of the reversals and twistings of the C line on Aug. 30th and Sept. 3rd, and the observation of the metallic prominence on Sept. 10, agree in placing the seat of greatest activity as occurring in the same portion of the following spot of the group. As the scope of the details just described is only to supplement the observation of the metallic prominence, all reference to the observations of the remaining lines between B–D in the spectrum of the group has been omitted, as they will, it is hoped, be more appropriately published elsewhere.

St. Beuno's College, St. Asaph, A. L. CORTIE.
1891, Oct. 17.

Saturn.

SEVERAL circumstances combine to make this planet an object of special interest at the present time. The ring was invisible from Sept. 22 to Oct. 30, but the northern side has just come into view and observers now have an opportunity of studying the extremely narrow ansæ. Are they perfectly smooth, continuous lines of light, or broken up by irregularities due to a real inequality of surface? When the ring is but a scarcely perceptible thread of light, as at present, it is usually seen under a beaded aspect and forms a delicate object in consequence of the very feeble (and perhaps irregular) reflective capacity of the outer edge.

In 1848 Schmidt observed numerous breaks in the ring, then presented nearly edgeways. On such occasions one ansa only has been sometimes visible, and the curious fact has been noticed by others that the ring extends further outwards on one side than on the other. The shadow of the ring thrown on the planet's bright equatorial zone has shown dark projections which can only be attributed to a mountainous character of the surface of the ring.

Peculiarities of this kind have been frequently remarked and are attested by observers of good reputation. But it is difficult to say how far the observations represent real features, for the effects of atmospheric disturbances upon a very delicate structure such as this must occasion false appearances precisely of the nature described. It is exceedingly difficult to discriminate between false and veritable details in an object so liable to distortion by the slightest tremors of the air. The narrow thread of light is constantly undulating and thus assumes a broken or a serpentine aspect as every wave of vapour passes over it.

When the Earth is nearly in the plane of the ring and there is practically no interference from the latter, the period is aptly suited for the observation of the planet's belts and spots. The belts are then seen crossing the disc in straight lines like the belts of Jupiter*, and their arrangement can be easily depicted. Markings if existing in or near the dark equatorial belts may then be seen to the best advantage. It was in Dec. 1876, when the rings were turned nearly edgeways, that Prof. Hall at Washington detected a white spot just N. of the equator of Saturn. The last opposition of the planet is also noteworthy, for a considerable number of equatorial spots have been observed by Mr. A. S. Williams, of Brighton, with a reflector of 6½ inches aperture, powers 225 and 320 (Huygenian) and 328 (single lens). He succeeded in obtaining glimpses of these markings early in May last, and practice soon gave his eye increased power, so that at the end of June they had become *easy objects* though the planet was less favourably situated. Some of the spots were followed during an interval of two months, and by unwearying efforts Mr. Williams managed to secure the estimated times of 80 of their transits across the central meridian of Saturn.

The following figures are reproduced from drawings by Mr. Williams. Fig. 1 (May 7) represents a bright spot (2 *a*) nearly at mid-transit, and it was preceded by a small oval dark spot (2) on the N. side of the S. equatorial belt. Fig. 2 shows the same markings as they were reobserved on May 13, when another white spot was detected nearing the W. limb. Fig. 3 (June 5) includes the pair of spots seen on May 7 and 13, and a white spot was now observed just preceding the dark spot. A peculiar broad, dusky streak was also noticed in the planet's N. hemisphere; its major axis was apparently placed at right angles to the direction of the belts.

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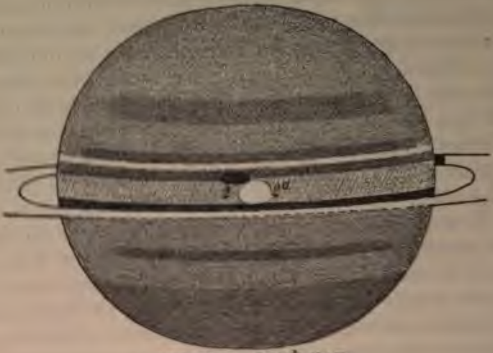
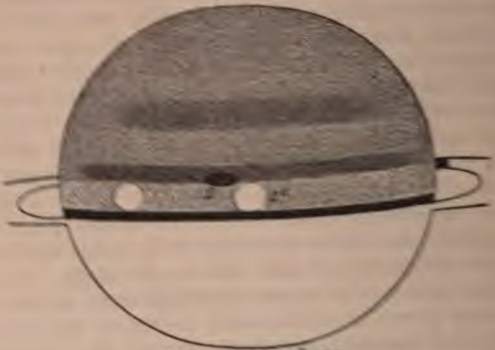
* This is not perhaps strictly true, but the inclination of Jupiter is so small that the curvature of the belts may be disregarded and it is rarely shown in drawings.

Fig. 1.



1891 May 7, 9h 50m.

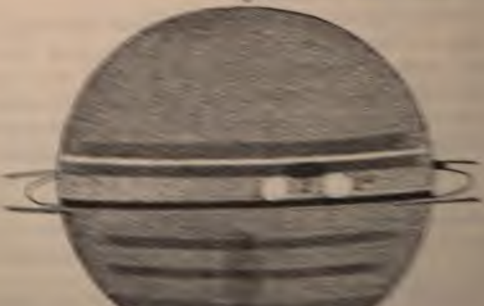
Fig. 2.



1891 May 18, 9h 35m.

(Details in N. hemisphere not depicted.)

Fig. 3.



1891 June 3, 9h 12m.

As these results rest on isolated testimony, some doubt may possibly be expressed concerning them. It is certain that the features alluded to were seen by no one else, though in several cases they were particularly looked for. Such markings have resisted the powers of the Lick 36-inch refractor, the Washington 26-inch, the Chicago 18½-inch, Mr. Green's 18-inch reflector, &c., &c., Is it possible to conceive, therefore, that they could be easily seen in a 6½-inch reflector? It is evident that by long practice and perseverance Mr. Williams has acquired phenomenal powers of observation, or that he has something more than a lively imagination. The times of transit which he gives for the individual spots agree in a remarkable manner amongst themselves and support the rotation period derived by Prof. Hall in 1877. This in itself is corroborative evidence of no light character, and I think it is far easier to accept the observations than to explain them away. No doubt it is another case where a capable observer has shown the great utility of a small telescope in distinguishing markings on a bright planet.

I have derived the rotation periods of 11 of the spots discovered by Mr. Williams. The number of rotations varied from 77 to 138. After correcting the apparent periods for differences in the relative longitudes of Saturn and the Earth and for equation of light, I found from 10 white equatorial spots a mean period of

$$10^{h}\ 14^{m}\ 26^{s}\cdot6.$$

The individual determinations varied between $10^{h}\ 14^{m}\ 19^{s}\cdot5$ and $10^{h}\ 14^{m}\ 33^{s}\cdot9$. For a dark spot situated on the equatorial side of the southern belt I found

$$10^{h}\ 14^{m}\ 24^{s}\cdot2.$$

Prof. Hall's period was $10^{h}\ 14^{m}\ 23^{s}\cdot8$ from his white equatorial spot of Dec.–Jan. 1876–7, while Sir W. Herschel deduced $10^{h}\ 16^{m}\ 0^{s}\cdot4$ from certain irregularities observed in a quintuple belt in 1793–4.

In 1880 I made a number of observations of Saturn with a 10-inch reflector and remarked that "the equatorial zone is the brightest region of the disk and gives indication of white patches in moments of best definition" ('Monthly Notices,' vol. xli. p. 82). I have at other periods suspected faint white spots here, but never followed them with desirable closeness. I feel confident that the markings seen by Mr. Williams will be reobserved during the ensuing apparition of Saturn. It is to be hoped that some of the large instruments will be employed in recovering and following them. There is no reason to consider these features as recent disturbances or that they have now disappeared. Probably they are very similar to the spots on Jupiter and are pretty durable atmospheric phenomena, though liable to certain variations from time to time.

With the confirmation of recent discoveries we shall enter upon what may be regarded as a new era in observations of this beautiful planet. Instead of the lack of variety in Saturnian detail as observed in former years we may find numerous instances of change and disturbance in the equatorial region of the planet, reminding us forcibly of the more striking phenomena exhibited on Jupiter. Observers with modest appliances will also recognize, more than ever, that their means are adequate to important work in the field of planetary observation.

Bristol. W. F. DENNING.

An Astronomer's Work in a Modern Observatory.

[Continued from p. 341.]

WE now come to an important class of astronomical work more purely astrophysical, for the illustration of which I can no longer appeal to the Cape, because I regret to say that we are not yet provided with the means for its prosecution. I refer to the use of the spectroscope in astronomy and especially to the latest developments of its use for the accurate measurement of the velocity of the motions of stars in the line of sight*.

It is beyond the province of this lecture to enter into history, but it is impossible not to refer to the fact that the chief impulse to astronomical work in this direction was given by Dr. Huggins, our chairman to-night—nay, more, except for the early contributions of Fraunhofer to the subject, Dr. Huggins certainly is the father of sidereal spectroscopy, and that not in one but in every branch of it. He has devised the means, pointed the way, and, whilst in many branches of the work he still continues to lead the way, he has of necessity left the development of other branches to other hands.

From an astrometer's point of view the most important advance that has been made in spectroscopy of recent years is the sudden development of precision in the measures of star motion in the line of sight. The method remained for fifteen or sixteen years quite undeveloped from the condition in which it left the hands of Dr. Huggins, and certainly no progress in the accuracy attained by Dr. Huggins was made till the matter was taken up by Dr. Vogel a Potsdam. At a single step Dr. Vogel has raised the precision of the work from that of observations in the days of Ptolemy to that of the days of Bradley—from the days of the old sights and pinnules to the days of telescopes. Therefore I take a Potsdam observation as the best type of a modern spectroscopic observation for description, especially as I have recently visited Dr. Vogel at Potsdam, and he has kindly given me a photograph of his spectroscope, as well as of some of the work done with it.

A photograph of the Potsdam spectroscope attached to the equatorial is now on the screen. [Description.]

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* The older methods enabled us to measure motions at right angles to the line of sight, but till the spectroscope came we could not measure motions in the line of sight.

The method of observation consists simply in inserting a small photographic plate in the dark slide, directing the telescope to the star, and keeping the image of the star continuously on the slit during an exposure of about an hour; and this is what is obtained on development of the picture. [Illustration: *see* Frontispiece.]

If the star remained perfectly at rest between the jaws of the slit the spectrum would be represented by a single thread of light, and of course no lines would be visible upon such a thread; but the observer intentionally causes the star-image to travel a little along the slit during the time of exposure, and so a spectrum of sensible width is obtained. (Fig. 1.)

You will remark how beautifully sharp are the faint lines in this spectrum. Those who have tried to observe the spectrum of Sirius in the ordinary way know that many of these fine lines cannot be seen or measured with certainty. The reason is that on account of irregularities in atmospheric refraction, the image of a star in the telescope is rarely tranquil, sometimes it shines brightly in the centre of the slit, sometimes barely in the slit at all, and the eye becomes puzzled and confused. But the photographic eye is not in the least disturbed; when the star-image is in the slit, the plate goes on recording what it sees, and when the star is not in the slit the plate does nothing, and it is of no consequence whatever how rapidly these alternate appearances and disappearances recur. The only difference is that when the air is very steady and the star's image, therefore, always in the slit, the exposure takes less time than when the star is unsteady.

That is one reason why the Potsdam results are so accurate. And there are many other reasons besides, into which I cannot now enter. What, however, it is very important to note is this, that we have here a method which is to a great extent independent of the atmospheric disturbances which in all other departments of astronomical observation have imposed a limit to their precision. Accurate astrospectroscopy, therefore, may be pushed to a degree of perfection which is limited only by the optical aid at our disposal and by the sensibility of our photographic plates.

And now I think we have sufficiently considered the ordinary processes of astronomical observation to illustrate the character of the work of an astronomer at night, the picture should be completed by an account of his work by day. But to go into the matter in detail would certainly not be within the limits of this lecture. It is better that I should in conclusion touch upon some recent remarkable results of these day and night labours. It is these after all that most appeal to you, it is for these that the astronomer labours, it is the prospect of them that lightens the long watches of the night and gives life to the otherwise dead bones of mechanical routine.

Let us take first some spectroscopic results. To explain their meaning let me remind you for a moment of the familiar analogy between light and sound.

The pitch of a musical note depends on the rapidity of the vibrations communicated to the air by the reed or string of the musical instrument that produces the note, a low note being given by slow vibrations and a high one by quick vibrations.

Just in the same way red light depends on relatively slow vibrations of ether, and blue or violet light on relatively quick vibrations. Well, if there is a railway train rapidly approaching one, and the engine sounds its whistle, more waves of sound from that whistle will reach the ear in a second of time than would reach the ear were the train at rest. On the other hand, if the train is travelling at the same rate *away* from the observer, fewer waves of sound will reach his ears in a second of time. Therefore an observer beside the line should observe a distinct change of pitch in the note of the engine-whistle as the train passes him, and as a matter of fact such a change of pitch can be and has been observed.

Just in the same way, if a source of light could be moved rapidly enough towards an observer it would become bluer, or if away from him it would become more red in colour. Only it would require a change of velocity in the moving light of some thousands of miles per second in order to render the difference of colour sensible to the eye. The experiment is, therefore, not likely to be frequently shown at this lecture table!

But the spectroscope enables such changes of colour to be measured with extreme precision. Here on the screen is the most splendid illustration of this that exists at present, viz., copies of three negatives of the spectrum of α Aurigæ, taken at Potsdam in October and December of 1888 and in March 1889. (Fig. 2.)

The white line (the picture being a positive) represents the bright line Hγ given by the artificial light of hydrogen, the strong black line in the picture of the star spectrum corresponds to the black absorption line which is due to hydrogen in the atmosphere of the star.

Why is it that the artificial hydrogen line does not correspond with the stellar line in these three pictures? The answer is, either the star is moving towards or from the Earth in the line of sight, or the Earth is moving from or towards the star. But in December the Earth in its motion round the Sun is moving at right angles to the direction of α Aurigæ; why, then, does not the stellar hydrogen line agree in position with the terrestrial hydrogen line? the simple explanation is that α Aurigæ is moving with respect to the Sun.

In what way is it moving? Well, that also is clear—the stellar line is displaced towards the red end of the spectrum, that is to say the star-light is redder than it should be in consequence of a motion of recession; this proves that the star is moving away from us, and measures of the photograph show the rate of this motion to be 15½ miles per second. We also know that in October the Earth in its motion round the Sun is moving towards α Aurigæ nearly at the same rate as we have just seen that α Aurigæ is running away from the Sun. Consequently, at that time, their relative motions are nearly insensible, because both are going at the same rate in the same direction, and we find accordingly, in October, that the positions of the stellar and artificial hydrogen lines perfectly correspond. Finally, in March, the Earth in its motion round the Sun is moving away from *α* Aurigæ, and as *α* Aurigæ is also running away from the Sun the star-light becomes so much redder than normal that the stellar hydrogen line is shifted completely to one side of the hydrogen and artificial line.

The accuracy of these results may be proved as follows :—

If we measure all the photographs of *α* Aurigæ which Dr. Vogel has obtained we can derive from each a determination of the relative velocity of the motion of the star with respect to our Earth.

Of course these velocities are made up of the velocity of motion of *α* Aurigæ with respect to the Sun (which we may reasonably assume to be a uniform velocity) and the velocity of the Earth due to its motion round the Sun. But the velocity of the Earth's motion in its orbit is known with an accuracy of about one five-hundredth part of its amount, and therefore within that accuracy we can allow precisely for its effect on the relative velocity of the Earth and *α* Aurigæ. When we have done so we get the following results for the velocity of the motion of *α* Aurigæ with respect to the Sun. You see by the following table (p. 374) how beautifully they agree in the Potsdam results, and how comparatively rough and unreliable are the results obtained by the older method at Greenwich.

I believe that in a few years, at least in a period of time that one may hope to see, we shall not be content merely to correct our result for the motion of the Earth in its orbit only, and so test our observations of motion in the line of sight, but that we shall have arrived at a certainty and precision of working which will *permit the process to be reversed,* and that we shall be employing the spectroscope to determine the velocity of the Earth's motion in its orbit, or, in other words, to determine the fundamental unit of astronomy, the distance of the Sun from the Earth.

I will take as another example one recent remarkable spectroscopic discovery.

Miss Maury, in examining a number of photographs of stellar spectra taken at Harvard College, discovered that in the spectrum of *β* Aurigæ certain lines doubled themselves every two days, becoming single in the intermediate days. Accurate Potsdam observations confirmed the conclusion.

The picture on the screen (Fig. 3) shows the spectrum of *β* Aurigæ photographed on November 22 and 25 of last year. In the first the lines are single, in the other every line is doubled. Measures and discussion of a number of these photographs have shown that the doubling of the lines is perfectly accounted for by the supposition of two suns revolving round each other in a period of four days, each moving at a velocity about 70 miles a second in its orbit.

α AURIGÆ—POTSDAM.

Date.	Observed Relative Motion of Earth and Star. Miles per sec.	Motion of Earth.	Concluded Motion, Star Relative to the Sun.
1888.			
October 22nd ...	+ 2·5	− 13·0	+ 15·5
„ 24th ...	+ 3·1	− 12·4	+ 15·5
„ 25th ...	+ 3·1	− 12·4	+ 15·5
„ 28th ...	+ 2·5	− 11·8	+ 14·3
November 9th ...	+ 6·8	− 8·7	+ 15·5
December 1st ...	+ 11·8	− 3·1	+ 14·9
„ 13th ...	+ 14·9	+ 0·6	+ 14·3
1889.			
January 2nd ...	+ 20·5	+ 6·8	+ 13·7
February 5th ...	+ 32·9	+ 14·3	+ 18·6
March 6th	+ 34·2	+ 16·8	+ 17·4

α AURIGÆ—GREENWICH.

Date.	Observed Relative Motion of Earth and Star. Miles per sec.	Motion of Earth.	Concluded Motion, Star Relative to the Sun.
1887.			
January 26th ...	+ 16·4	+ 12·6	+ 3·8
February 16th ...	+ 34·4	+ 15·9	+ 18·5
October 22nd ...	+ 39·8	− 13·5	+ 52·3
„ 25th ...	+ 25·4	− 13·0	+ 38·4
„ 29th ...	+ 40·6	− 12·1	+ 52·7
1888.			
December 7th ...	+ 29·0	− 1·2	+ 36·2
1889.			
February 15th ...	+ 23·8	+ 16·0	+ 7·8
March 5th	+ 20·3	+ 17·1	+ 3·2
September 17th ...	+ 18·6	− 13·3	+ 33·3
„ 19th ...	+ 21·8	− 16·7	+ 38·5
„ 25th ...	+ 24·8	− 16·5	+ 41·3
November 25th ...	+ 24·5	− 4·9	+ 29·4

When one star is approaching us and the other receding, the lines in the spectrum formed by the light of the first star will be moved towards the blue end of the spectrum, those in the spectrum of the second star towards the red end of the spectrum. Then, as the two stars come into the same line with us, their motions become at right angles to the line of sight, and their two spectra, not being affected by motion, will perfectly coincide; but then, after the stars cross, their spectra again separate in the opposite direction, and so they go on.

Thus by means of their spectra we are in a position to watch and to measure the relative motions of two objects that we can never see apart; nay more, we can determine not only their period of revolution but also the velocity of their motions in their orbits. Now, if we know the time that a body takes to complete its revolution, and the velocity at which it moves, clearly we know the dimensions of its orbit, and if we know the dimensions of an orbit we know what attractive force is necessary to compel the body to keep in that orbit, and thus we are able to weigh these bodies. The components of β Aurigæ are two suns, which revolve about each other in four days; they are only between 7 and 8 millions of miles (or one-twelfth of our distance from the Sun) apart, and if they are of equal weight they each weigh rather over double the weight of our Sun.

I have little doubt that these facts do not represent a permanent condition, but simply a stage of evolution in the life-history of the system, an earlier stage of which may have been a nebular one.

Other similar double stars have been discovered both at Potsdam and at Cambridge, U.S., stars that we shall never see separately with the eye aided by the most powerful telescope; but time does not permit me to enter into any account of them.

David Gill.

[To be continued.]

The Solar Disturbances of June 17, 1891.

With reference to Mr. H. H. Turner's very interesting paper, published in the last number of the 'Observatory,' p. 326, I will state that the bright spot observed June 17th already existed when I first noticed it at 10^h 16^m A.M. Paris mean time. Very shortly after this time, the facula-like object seen close to the limb very rapidly lighted up, beginning at the extremity nearest to the other bright spot first observed, thus showing that the propagation of activity was northward from that spot.

When the Sun's image was projected on the screen for the second time, nothing which could be seen remained on that particular part of the Sun where the phenomena had been observed, not even the facula-like object seen before.

In the hurry of the observation this facula, which very likely had resumed its ordinary brightness, might perhaps have escaped my notice. But this is rather doubtful since my son, who was present and observed the phenomenon on the screen with me, also failed to detect any trace of faculæ or any other things.

I will call the editor's attention to a slight mistake on p. 328, where the word "Sept.," under the engraving, should be replaced by "June." E. L. Trouvelot.

Meudon Observatory,
1891, Oct. 12.

Double Stars for November.

THE following interesting double stars are well placed for observation in November. They may all be observed with a telescope of moderate aperture. The positions are for 1890·0.

Σ 3062. R.A. $0^h\ 0^m\ 80^s$, Dec. +57° 49′; Mag. 6·9, 8.

A binary star. Period about 112 years (Schur). One revolution has now been nearly completed. Some recent measures are:—

1887·10	310°·73	1″·50	Tarrant.
1889·57	321 ·1	1 ·45	Burnham.

η Cassiopeiæ. R.A. $0^h\ 42^m\ 27^s$, Dec. +57° 14′; Mag. 4, 7·6.

A well-known binary star. Period about 195 years (Grüber).

1886·974	178°·64	4″·71	Tarrant.

36 Andromedæ (Σ 73). R.A. $0^h\ 49^m\ 5^s$, Dec. +23° 2′;
Mag. 6·2, 6·9.

Binary. Period 137·5 years (Lewis). Some recent measures are:—

1888·05	7°·7	1″·18	Schiaparelli.
1888·51	6 ·1	1 ·21	Leavenworth.
1891·08	9 ·2	1 ·35	Maw.

α Piscium. R.A. $1^h\ 56^m$, Dec. +2° 12′; Mag. 3·7, 4·7.

In slow motion.

1886·95	321°·87	3″·03	Tarrant.

Double. Hastings. R.A. $2^h\ 11^m$, S. 18° 44′; Mag. 8·3, 8·9.

In rapid motion. The measures are:—

1879·92	311°·8	2″·22	Hall.
1888·66	339 ·6	2 ·18	Leavenworth.

Measures of this star are very desirable. It seems to be Lalande 4219 (8½ mag).

84 Ceti (Σ 295). R.A. $2^h\ 36^m$, S. 1° 10′; Mag. 6, 10.

In slow motion.

1888·06	322 ·2	4″·50	Tarrant.
1888·68	323 ·0	4 ·27	Leavenworth.

μ Draconis (Σ 2130). R.A. $17^h\ 3^m{\cdot}3$, Dec. +54° 37′; Mag. 5, 5.

A slow-moving binary. Period 648 years, according to Berberich. Plane of orbit at right angles to line of sight.

1886·73	161°·9	2″·51	Engelmann.
1886·94	160 ·0	2 ·35	Tarrant.

J. E. GORE.

Selenographical Notes.

Bode, Pallas, and Murchison.—There are few lunar landscapes that present a more striking and beautiful picture than that which includes this group and the neighbouring mountainous region, if it be observed a few hours after sunrise with a moderately high power. Neison justly remarks that there is much in it to remind us of terrestrial formations. Certainly, the branching hill-ranges, with their associated valleys and other features, are of a type more frequently met with on the Earth than on the Moon's surface, a circumstance which is also in a great measure true as regards the character of the country which forms the gentle south-western slope of the neighbouring Apennines and of the intervening plain. The very brilliant boundaries of the three formations contrast strongly with the sombre hue of the hill and the valley region on the north-west under oblique illumination, as do also the many little white isolated heights which lie scattered on the grey surface on the south of Murchison. Like other objects situated amid the rugged details of lunar mountain-districts, the group with its adjuncts is very difficult to portray, owing to its complexity. The representation of it in Schmidt's map is on a sufficiently large scale to allow the insertion of details which cannot be distinctly shown in those of Mädler and Neison, and, although not wholly satisfactory, it is generally truthful, especially as regards the mountain-land by which the group is environed.

Pallas is encircled by an all but continuous rocky border, while Murchison, its neighbour on the west, is to a great extent open to the plain on the south, the bright ring-plain, Murchison A, marking the eastern termination of the long stretch of curved wall which forms its boundary on the north and west. The structure of this barrier is noteworthy. About one half of it, the portion adjoining Pallas, is not only far less brilliant than the remainder, but displays a number of long, narrow, parallel spurs or buttresses running out from it at right angles towards the central region of the floor. These are well seen during the progress of sunrise, standing out as faint grey bands amid the black shadow of the lofty border, which, according to Neison, rises some 3000 feet above the interior. The rest of the wall, up to Murchison A, is both wider and far more brilliant, and exhibits two curious longitudinal depressions abutting on the north side of A, resembling the three prongs of a fork. This portion of the border is less lofty than the other and rises directly from the plain, while the more easterly and darker section appears to be little more than a steep cliff termination to the mountain region which bounds it on the north. The opposite, or southern, side of Murchison consists of a narrow longitudinal strip of bright wall terminating on the west in two other strips of about equal length and brightness, spreading out like the sticks of a fan. The first, the massive mountain-arm *a* (Neison, Map X.), and the broad complex western wall of Pallas form a cross, near the centre of which, on the northern end of *a*, stands a very deep and brilliant crater. The floor of Murchison exhibits no detail beyond three or four low ridges.

Perhaps the most noticeable structural feature remarked by the observer who examines Pallas and Murchison under a rising sun is the bright wall which strikes across the two formations from north to south, forming the division between them and extending some distance beyond their limits on either side. The mountain-arm *a* is the southern prolongation of this partition, and on the north a similar rocky mass (β) traverses the dusky mountain-land for many miles. That portion of the wall which constitutes the boundary common to the two ring-plains exhibits two or three narrow gaps and is only very slightly curved, the concavity facing the interior of Pallas. Hence this formation approximates in shape to the letter **D**, the straight part of the letter representing that section of the transverse wall which forms the partition.

The north-east border of Pallas is broad and, except under a high light, very brilliant. At 9^h 30^m on April 16, this year, a well-marked valley or terrace was noted running round the inner slope of it. At this time Bode and the smaller crater south of it on the wall were only partially illuminated. Two other much shallower depressions were seen north-west of the last, standing side by side, on the border. Schmidt shows a large number of mounds and a long low ridge on the north-western side of the floor. With the exception of the latter, which was very evident, I was unable to verify the existence of these details, the only prominent objects being a large bright circular mountain, a little south of the centre (the shadow of which at 10^h extended up to the foot of the eastern wall), and a much smaller bright hill adjoining it on the south. At times a crater was suspected on the central mountain. Schmidt shows a central mountain in Bode, and Webb noticed a curved ridge traversing it from north to south. The former is not a difficult object, but the existence of the ridge has not been verified.

Neither Pallas nor Murchison are easily traceable under a high sun, though Bode, Pallas A, Murchison A, and, in a less degree, the little crater at the centre of the cross referred to above, standing out as very brilliant rings, serve to mark their position. The notable difference in brilliancy at full Moon between objects which under lower angles of illumination are, roughly speaking, of equal brightness, is a subject which has not received the attention it deserves. Is it not due to variations both in colour and mineral character of the rocks of which they are composed?

Beaumont House, Shakespeare Road,
Bedford, 1891, Oct. 19.

THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Real Path of a fine Meteor.

Gentlemen,—

On September 30 at $10^h\ 2^m$, while observing Temple-Swift's periodical comet, I saw a meteor of about 1st mag. moving slowly downwards between ζ Draconis and γ Ursæ Minoris. As it fell it rapidly increased in brilliancy, and just prior to its disappearance it must have been twice as bright as Venus. It quite lit up the northern sky for a moment. The nucleus was of a bluish-white colour, but the streak it left was not nearly so dense and durable as one would expect from so bright an object; it merely consisted of a line of thin white vapour that died away in 2 seconds. The observed path of the meteor was from $263^\circ +73^\circ$ to $217\frac{1}{2}^\circ +54^\circ$, length 26°, but there is no doubt I missed the early part of the flight. Its duration was estimated as $3\frac{1}{2}$ seconds.

A communication from Mr. D. Booth, of Leeds, received a few days later, informed me that he had seen a large meteor, brighter than Jupiter, on September 30, at $10^h\ 3^m$, passing from east to west amongst the stars of Capricornus. The exact places of beginning and ending where $320^\circ -15^\circ$ and $300^\circ -20^\circ$, length 20°, and the path if prolonged backwards would pass 2° above the planet Jupiter, then near the meridian. The meteor moved slowly, duration 3 seconds, and it left a thin train or streak.

Comparing the two observations I find the real path of the body was from a height of 54 miles over Lydney in Monmouthshire to 24 miles over Builth in Breconshire. The earth-point was near Aberystwith, and the entire length of path, as observed at Leeds, 56 miles. The velocity, from a mean of the two observations, was 13 miles per second.

As to the radiant-point, that is well indicated at $14^\circ +7^\circ$ near ϵ Piscium, situated in azimuth 314° W. from S., altitude 35° at the time of the observation. The meteor probably belonged to the following (possibly) associated showers:—

Date.	Radiant.	Notes.	Ref. No.
1885 Sept. 13–17..	$13^\circ + 6^\circ$	V. accurate; swift meteors.	D. 530
1886 Sept. 22–30..	$14 \ +12$	Not exact; slow, v. brilliant trained meteors. Max. Sept. 27.	D. 597
1887 Oct. 11	$13 \ + 6$	Accurate; slow, v. bright trained meteors.	D. 643

The figures in the last column refer to my catalogue of Radiant-points in the 'Monthly Notices' for May 1890. This shower near ϵ Piscium appears to have developed exceptional activity in recent years and to supply a large proportion of fireballs.

Mr. Booth estimated the meteor as brighter than Jupiter, while I describe it as twice as bright as Venus. The cause of the difference is easily explained. At the middle of its flight the meteor, though not more than 42 miles distant from Bristol, was 173 miles distant from Leeds, and as the amount of light received from an object is diminished as the square of the distance is increased, it is apparent that to a Bristol observer it would appear fully 16 times as bright as to an observer at Leeds. Moreover, I saw the object from a dark neighbourhood and in a dark sky, whereas Mr. Booth's station was much illuminated with gas, and thus the effects of contrast were lost. As seen from Bristol, the meteor was of small angular diameter and star-like in appearance, but intensely brilliant just before it terminated its career near θ Boötis.

Yours faithfully,
Bristol, 1891, Oct. 8. W. F. DENNING.

P.S.—Another very fine meteor was observed at several stations on Sunday, Oct. 11, $6^h\ 30^m$. Mr. Arthur Mee, at Ammanford, Caermarthenshire saw the meteor, though the sky was overcast with clouds. He says that it appeared in "the south, about two thirds of the way from the horizon to the zenith, and moved quickly over perhaps a dozen degrees from east to west." During its flight "behind the curdled cloud-masses it flashed several times, just like lightning, only fainter." Mr. E. M. Richards of Enniscorthy, Ireland, describes the meteor as much larger and brighter than Jupiter and of a bluish-white colour. It pursued a nearly horizontal path of about 40°, and its first apparition was noted in azimuth 275° W. from S., altitude 20°. It disappeared in a shower of sparks at an altitude of 15° after a duration of 4 seconds. Other correspondents describe the meteor as remarkably brilliant, lighting up the sky like a flash of lightning. It is hoped that further data will be forthcoming, so that its real course may be determined.

W. F. D.

PUBLICATIONS.

ON THE ADJUSTMENT AND TESTING OF TELESCOPIC OBJECTIVES. T. COOKE & SONS, York.—Messrs. T. Cooke & Sons have issued a small book on the Adjustment and Testing of Telescopic Objectives, which will be welcome to all who use achromatic telescopes, and more especially to those who are far from Europe or for other reasons are unable to avail themselves of the knowledge and skill of professional opticians. There is no one probably who has found himself thrown on his own resources to deal with a telescope, who has not felt how unsatisfactory the book information available as to the subjects here treated was: many have tolerated defects of vision which might have been remedied lest they should do more harm than good; some have condemned good tools; and, a more serious matter, some have mistaken the results of defective vision in the combination of eye and telescope for discoveries and wasted their own time if not that of others. To all such, Messrs. Cooke's book will be most useful, unless they are of the class of Mr. Troughton's "handy gentlemen," who may be tempted to tamper with their instruments instead of letting well alone. To these, too, Messrs. Cooke's little book will offer every facility, and the result will probably be good for trade and possibly for Astronomy.

This little book points out what is not generally made so clear, that vision with an Astronomical Telescope is the result of three separate parts—the object-glass, the eyepiece, and the eye, all of which are liable to defects; and of these the last, which is usually least suspected, is perhaps the most liable to be imperfect. This is not new, but it can never be too much insisted on. Inasmuch as we have no knowledge of what good vision is save by our own experience, we are prone to jump to the conclusion that there is no better sight than our own, and to ascribe to objects we are looking at, or the telescope through which we look, appearances which are in fact due to our own imperfect eyes. Somewhere about a quarter of a century ago the writer of this article, after describing his own experience, suggested that it might be "necessary to make every observer of delicate phenomena *prove that he could see*;" and recent discussions show that the advice may be repeated with advantage *.

The most common error that the observer is called on to deal with is that of "squaring on," as Mr. Cooke calls it, that is, making the optical axis of the objective coincide with that of the eyepieces. The instructions are clear and good, but it is to be hoped that the form of antagonizing (*agonizing* I used to hear them called once) screws is not Messrs. Cooke's ideal. It is wonderful that some ingenious person has not devised a better arrangement for holding the countercell to the tube, while retaining a power of adjustment, than antagonizing screws; but surely the plan of tapping the holding-down screw through the thrusting-screw is better and firmer than that shown in the figures on p. 7.

Under the head of Achromatism, Messrs. Cooke point out the sense in which the Huyghenian eyepiece is achromatic and the effects of want of achromatism in the eye. They point out that the Ramsden eyepiece as well as the Huyghenian produces but very slight colouring effects; but it might have been as well to say that even in the sense in which the Huyghenian eyepiece is achromatic the Ramsden is not so. Nothing is said of the achromatic eyepieces of several makers; doubtless because when a telescope is overcorrected for colour for use with the usual eyepieces there is no gain from their use; but in issuing a reprint of this little book, which will probably soon be called for, it might be worth giving some information as to the forms and properties of the different eyepieces which have been proposed. Observers generally add to the battery of eyepieces supplied by the optician, and the recommendations of authorities are so liable to be extended to cases which they did not contemplate, and thus cause disappointment, so that some information as to these forms would be useful.

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* On p. 21 Messrs. Cooke say that in many individuals astigmatism is found to be variable in amount, an eye almost free from the defect one day being seriously affected another. This is hardly that malformation of the lenses of the eye usually called "Astigmatism." It is understood to arise from spasm of the ciliary muscles; and it is very doubtful whether the remedy suggested would make the observations with such an eye trustworthy. The writer's own experience tells him that under certain circumstances, such as fatigue, there is to him, as probably to every one, temporary difficulty in seeing clearly, and that there seems to be an occasional shifting necessary of the axis of the cylindrical surface of the lenses he uses, but nothing which resembles Messrs. Cooke's description.

The authors are evidently partial to the Achromatic Telescopes for which the firm has been so long celebrated, and hardly do the Reflector justice. It may be quite true that with ordinary eyepieces the Reflector is not perfectly achromatic, but those do not give it fair play. Achromatic eyepieces have always, it is believed, improved the performance, and there seems to be no reason why they should not be generally used. So, too, the Reflector requires exceptionally fine conditions to bring out its powers; but, when those are present, people familiar with both kinds of telescope do not consider them inferior. The Achromatic will always be preferable for general use and indispensable for some purposes. The choice is not only a question of price of the objective; the object-glass of an achromatic telescope is generally but a fraction of the cost of a complete equatorial, and the users of reflectors are usually content with fewer conveniences and less showy workmanship, by which much is saved without loss of quality in the work.

Again, Messrs. Cooke do not do justice to the knife-edge test. It was one of the tests proposed and used for specula by Foucault; and these have given the best results, not only in his hands, but in those of the MM. Henry of Paris, who, it is believed, use them in precisely Foucault's way for their reflectors. Now MM. Henry, who have made many fine objectives, including those for the Coudé Equatorials and the Photographic Telescope, cannot be dismissed as amateurs who make their own telescopes. M. Foucault asserted that with his tests he could construct mirrors so true in figure that they passed the limits of useful accuracy; the convergence of the rays being such, that from the nature of light, it was impossible to detect further errors.

But with all this the little book will be found full of information as to things which should be, though they are not, generally known by observers; and Messrs. Cooke will have the cordial thanks of astronomers, whether professional or amateur.

J. F. T.

PHOTOGRAPHS OF THE MILKY WAY AND NUBECULÆ.—Mr. Russell is responsible for a charming new departure in Observatory publications, in the shape of a photographic album. Sixteen prints of plates taken with one of Dallmeyer's largest portrait-lenses (6 inches aperture and 32 inches focal length) have been carefully mounted on the pasteboard leaves of an album, a few lines of description being added in each case. The photographs were taken in order to make some use of the equatorial mounting for the Photographic Telescope which was received some time before the object-glass. "They are believed to be the first of their kind of the southern skies, and they present well-known star-spaces under new conditions, so much so, that in several important respects we must modify the views which had been the outcome of the study of these star-spaces with ordinary telescopes." The plates were taken on fine nights (a haze only just visible made it necessary to give double the time of exposure) with an exposure of about 3 hours. We may perhaps quote a few of the remarks made on the photographs, as indicating the sort of conclusions which Mr. Russell has drawn from them. For the justice of these conclusions we must refer our readers to the album itself.

Photo No. 2. "It is as if lines of stars and dark spaces—not straight, but slightly curved—converged from the following side to a point 1¼ inches below the central star."

No. 3. "The Great Coal-sack is almost as full of stars as the parts around it."

No. 6. "The great rift (so-called) in the Milky Way, which comes up to, if it does not include, α Centauri when viewed with the telescope, is not found in this photograph."

No. 8. "The almost uniform distribution of stars over this area is somewhat at variance with the best drawings of the Milky Way."

No. 10. "In the brightest parts the stars seem to be almost in contact, so close together do they stand.....It will be observed that there are here several long narrow lanes or dark spaces in the Milky Way, and they occur so frequently in other pictures of the Milky Way that they may be taken to be a feature of it; but I cannot see that they are curved or straight in any systematic manner; they are rather meandering and irregular."

Nos. 12, 13, & 14. The Nubecula Major is found to be distinctly spiral in character.

The last photograph in the volume is of the Orion Nebula with the 13-inch object-glass, and gives considerable detail in the fainter portions. The brighter parts are of course over-exposed in the 3 hours given to the plate.

Philips' Patent Orrery is a card planisphere with the objects marked strictly limited to the ecliptic, and provided with two radial brass scales for reading-off N.P.D.'s. For rough determinations of rising and setting the instrument should be very handy. We hardly think the buttons which slide along the scales are well devised. All slides should be relatively long in the direction of sliding, and these buttons are circular in section.

NOTES.

Personal Equation.—M. Stroobant made some interesting experiments at the beginning of this year, with M. Wolf's personal equation machine, at the Paris Observatory. He found:—

(1) A difference of $+0^{s}{\cdot}07$ in chronographic and $+0^{s}{\cdot}04$ in eye and ear transits, between his personalities for opposite directions of apparent motion.

(2) That the influence of the degree of illumination of the field was insensible. (No mention is made of the number of observations on this point, and the results do not appear to be quite conclusive.)

(3) A sensibly constant personal equation of $-0^{s}{\cdot}14$ for the first limb, and $+0^{s}{\cdot}03$ for the second limb, of disks representing planets of all sizes from the Moon to Uranus. [There is also another curious difference between the limbs on which M. Stroobaut has not remarked, viz. that there is a difference of $0^{s}{\cdot}06$ between direct and inverse motion for the first limb, but no such difference for the second.]

(4) That his personal equation decreases algebraically (registration is made earlier and earlier) during a long series of observations, at the rate of about $0^{s}{\cdot}01$ per hour.

(5) "When making chronographic observations, it has often happened to me to note that the registration of a transit at a particular wire was too soon or too late. These remarks amounted in all to 82. I thought of examining how far these estimations were correct. For this purpose I compared the value of P.E. for the wire in question with its mean value for the five wires. I thus found that there were 69 right estimations and 13 wrong. I had also roughly indicated in such notes the amount of the error. On analyzing the results I find the following mean differences:—

"Noted 'Much too soon (or late)'	$+0^{s}{\cdot}058$	9 obs.
„ 'Too soon (or late)'	$+0{\cdot}041$	47 „
„ 'A little too soon (or late)'	$+0{\cdot}017$	26 „

(the sign + indicating that the difference was in the direction of the remark)."

(6) M. Stroobant finds a well-marked "decimal equation," both in eye and ear observations and in reading-off chronographic observations by estimating the tenths. The figure 0 occurs oftenest and 9 most seldom, the ratio for these two digits being about 3 to 1 in both cases.

Prof. Bigelow's Theory of the Solar Corona.—We were somewhat unjust to Prof. Bigelow in mentioning Prof. Nipher's criticism of his Theory (No. 179, p. 316) without adding that the criticism has been fully replied to. A short paper in the 'American

Journal of Science' for June 1891 is sufficient to dispose of Prof. Nipher's objections. The main point is that the Theory is a purely geometrical one, and does not claim to rest on any physical basis at present. Prof. Bigelow believes that the coronal streamers follow definite curves, for which he gives the equations, but has not ventured to attribute the phenomena to even a proximate cause. "We have avoided speaking of the apparent coronal structure as a phenomenon of electricity, in deference to the doubt that free electricity can exist at such high temperatures on the Sun's surface, but have shown that some force is present acting upon the corona according to the laws of electric potential."

A CATALOGUE OF RUTHERFURD'S PHOTOGRAPHIC PLATES OF THE SUN, THE MOON, AND THE STARS.—Our readers will doubtless be interested by the following extract from the 'Annals of the New York Academy of Sciences,' vol. vi.:—

"On November 13th, 1890, Lewis M. Rutherfurd, LL.D., of New York City, presented to the Columbia College Observatory all his photographic negatives taken between the years 1858 and 1878. Many of the plates had been measured, and these measures were included in the gift. The measures fill some thirty quarto volumes. Several years ago (1884) Mr Rutherfurd gave to the Observatory his 13-inch equatorial with its photographic corrector, with which the photographs, taken since 1868, were made; and at the same time he donated to the Observatory his measuring engine supplied with a *glass scale*. This improved measuring apparatus was used in January, 1872, and subsequently.

"The star cluster negatives received an exposure of either three or six minutes. Each negative bears the date, time of exposure, a record of the barometer and thermometer readings, and other data necessary for the reductions.

"The Observatory of Columbia College hopes to issue soon the reductions of many of the measures."

Then follows a catalogue of these negatives drawn up by Messrs. J. K. Rees and H. Jacoby. There are 359 solar, 436 lunar, 701 stellar, and 37 miscellaneous photographs.

THE TOTAL LUNAR ECLIPSE OF 1891, NOVEMBER 15.—Shortly before the total eclipse of the Moon of Oct. 4, 1884, the suggestion was made by Dr. Döllen, of Pulkowa (now of Dorpat), that a determination of the Moon's diameter unaffected by irradiation might be obtained by observing both the disappearance and reappearance of as many small stars as possible during totality, and with this object he prepared a list of stars near the Moon's place, and computed the times of occultation for various observatories. The next total eclipse visible in Europe was on

Jan. 28, 1888, and on that occasion Dr. Döllen again prepared and circulated lists. A considerable number of occultations were observed at these two eclipses, but as the weather was unfavourable at many stations, it is desired to renew the observations at the forthcoming total eclipse of Nov. 15 in the present year. With this end in view, MM. Scheiner and Blumbach, of Potsdam, have taken four photographs of the region where the eclipsed Moon will be. These plates have been measured by M. Renz, of Pulkowa, who published in No. 3061 of the 'Astronomische Nachrichten' a list of the places of 240 stars down to the eleventh magnitude which will be occulted at some part of the Earth during the eclipse. Of these, Dr. Döllen has selected 138, which will be occulted at some known observatory. He has computed the times and position-angles of disappearances and reappearances for a great number of observatories, the following being the list of those in the British Isles that have received them:—

Name of Observatory.	Longitude.				Latitude.		
	h	m	s		°	′	″
Armagh	0	26	35·5	W.	54	21	13
Birkdale	0	12		W.	53	38	
Birr Castle	0	31	40·9	W.	53	5	47
Cambridge	0	0	22·8	E.	52	12	52
Dublin	0	25	22	W.	53	23	13
Coleby Field (Wimbledon)	0	1		W.	51	27	
Durham	0	6	19·8	W.	54	46	6
Edinburgh	0	12	43·6	W.	55	57	23
Fernhill	0	8	0	W.	52	13	0
Glasgow	0	17	10·6	W.	55	52	43
Greenwich	0	0	0		51	28	38
Halifax	0	8		W.	53	44	
Harrow	0	1	19·9	W.	51	34	47
Liverpool	0	12	17·2	W.	53	24	4
Markree	0	33	48·4	W.	54	10	32
Oxford (Radcliffe)	0	5	2·6	W.	51	45	36
Stonyhurst	0	9	52·7	W.	53	50	40
Tulse Hill	0	0	27·7	W.	51	26	47

Anyone else who may desire to assist should apply for details of the occultations at their station to the nearest observatory on the list.

The region of totality is unfortunately somewhat barren of stars: thus, in the 1884 eclipse the list for Greenwich contained 36 phenomena, of which 16 were observed; in 1888 the list contained about 30, of which 15 were observed. The present list for Greenwich only contains 18 phenomena, but some additional ones which take place some time before totality may possibly be observed.

A. C. D. C.

THE THEORY OF JUPITER AND SATURN.—Prof. G. W. Hill contributes an article to the 'Astronomical Journal' (No. 247), of which the following is the substance:—

"The discussion of the observations of Jupiter and Saturn, now in progress in the office of the 'American Ephemeris and Nautical Almanac,' has reached such a stage that we can exhibit the results in reference to the correction of the mean longitude of Jupiter as it is given in my new theory of Jupiter and Saturn ('Astronomical Papers of the American Ephemeris,' vol. iv. p. 588). Dividing the material into eleven groups, roughly corresponding to as many revolutions of the planet, values of δL were obtained, which were shown in the following table:—

Interval.	Mean Year.	δL.	Weight.
1750–1765	1758	$-0''{\cdot}50+0{\cdot}137\,\mu$	4·2
1766–1777	1772	$-0\ {\cdot}71-0{\cdot}037\,\mu$	1·4
1778–1789	1783	$-0\ {\cdot}52-0{\cdot}063\,\mu$	0·9
1790–1801	1796	$-1\ {\cdot}28-0{\cdot}111\,\mu$	2·8
1802–1813	1808	$-1\ {\cdot}33+0{\cdot}133\,\mu$	5·5
1814–1825	1820	$-1\ {\cdot}30+0{\cdot}040\,\mu$	6·1
1826–1837	1832	$+0\ {\cdot}01-0{\cdot}097\,\mu$	11·7
1838–1849	1844	$-0\ {\cdot}04-0{\cdot}098\,\mu$	16·7
1850–1861	1856	$+0\ {\cdot}02-0{\cdot}068\,\mu$	16·6
1862–1873	1868	$+0\ {\cdot}28+0{\cdot}179\,\mu$	16·5
1874–1887	1881	$+0\ {\cdot}16+0{\cdot}139\,\mu$	19·0

"In the values of δL the modifications caused by a change in the mass of Saturn are shown by the terms involving μ, an indeterminate so chosen that $\mu = 1''$ corresponds to an augmentation of Bessel's mass $\frac{1}{3501{\cdot}6}$ by a thousandth part. The column of weights is added, that some idea of the degree of precision of the several values of δL may be obtained.

"These values can be very well represented by a linear function of the time, with the exception of the three belonging to the interval 1790–1825, which have abnormally large negative values. And the latter cannot be brought into harmony with the rest by assigning to μ any possible value; in fact, the solution of the equations gives for μ an insignificant quantity.

"The disagreement might be attributed to personal equations in the observers; but this seems not likely, as the three values of 1796, 1808, and 1820 are closely coincident, and yet are founded on material coming, in the first from Greenwich and Palermo, in the second from Greenwich, Palermo, and Paris, and in the third from Greenwich, Paris, and Königberg.

"Again we may suppose that there is some inequality of long period in the mean longitude of Jupiter not taken into consideration in the theory. If this arises from the mutual action of Jupiter and Saturn, the observations of the latter should more clearly exhibit the effects of this perturbation. However, there does not seem to be any indication of an inequality in Saturn two and a half times as great as that which would seem to have place in Jupiter.

"In order that nothing might be wanting to the clearing up of this difficulty, I determined to compute the terms in the great inequalities which depend on three times the principal argument, that is to say, on the argument $15g'-6g$. These terms have hitherto been neglected, and from induction one would suppose their coefficients were at the limit of smallness, permitting their being passed over, at least if regard is had only to the representation of the observations. The period of these terms is about 310 years, and their coefficients are seemingly in the neighbourhood of $0''\cdot1$."

Prof. Hill then works out the terms in question, finding ultimately that the mean longitude of Jupiter ought to be increased by the terms

$$\begin{aligned} n\delta z = &+0''\cdot115 \sin(15g'-6g-246''\,t+201^\circ\cdot3) \\ &+0''\cdot043 \sin(15g'-7g-120''\,t+308^\circ), \end{aligned}$$

and the mean longitude of Saturn by the terms

$$\begin{aligned} n'\delta z' = &+0''\cdot286 \sin(15g'-6g-246''\,t+21^\circ\cdot3), \\ &+0''\cdot212 \sin(14g'-6g-180''\,t+55^\circ\cdot6), \end{aligned}$$

and concludes that the long-period inequality for Jupiter is not of a magnitude sufficient to remove the difficulty stated at the beginning of this article. However, it appears worth while to have computed these inequalities, if the only result is that the doubt is removed.

THE FIRST SATELLITE OF JUPITER is, according to the observations of Prof. Schæberle and Prof. Campbell during August and September, "ellipsoidal, with one of its longer axes directed towards the centre of Jupiter. The other satellite appears to be spherical." This telegram of Prof. Holden's is, like many telegrams, not quite clear. Are we to understand that the time of revolution and of rotation of the satellite are identical, as in the case of our Moon? or should we read spheroidal for ellipsoidal? the *plane* of the two equal axes being directed towards the centre of Jupiter.

COMET NOTES.—In communicating a fine series of observations of Comet 1889 I. (Barnard, 1888, September 2) to the 'Astr. Journal,' a series extending over two years, Prof Barnard remarks that what appeared to be an anomalous tail to this Comet in 1889, June, has been explained by Prof Bredichin as simply an effect of perspective due to the great distance. This comet was, in fact, observed at a distance exceeding the aphelia of nearly all the short-period comets, and suggests quite new possibilities in cometary observation. It was searched for carefully in 1891, but could not be found with the 36-inch telescope of the Lick Observatory. The last observation is dated 1892, September 7.

Comet 1891 I. (Barnard-Denning) was observed at Windsor (N. S. Wales) by Mr. Tebbutt up to July 3, at the Cape by Mr. Finlay up to June 15, when its declination was —46°, and at Cordoba till July 9, "when its light had diminished so much that it was lost under the faintest illumination of the micrometer threads."

The orbit of Comet 1890 II. has been investigated by Herr Bidschof, who finds it slightly hyperbolic. In the autumn this year the comet will be only one-tenth the brightness of the last opposition; but as it may still be picked up, Herr Bidschof gives the following ephemeris (Ast. Nach. no. 3059):—

	R.A.			Decl.	
	h	m	s	°	′
Oct. 31..	10	10	26	+12	21·9
Nov. 4..	10	10	22	+12	10·5
8..	10	10	9	+12	0·1
12..	10	9	45	+11	50·7
16..	10	9	11	+11	41·9
Nov. 20..	10	8	26	+11	34·1
24..	10	7	30	+11	27·2
28..	10	6	21	+11	21·2
Dec. 2..	10	5	0	+11	16·0

Tempel-Swift's periodical Comet was picked up by Prof. Barnard on September 27, and independently by Mr. Denning (whose capacity for seeing faint objects with a comparatively small instrument seems to be remarkable) on September 30. Prof. Barnard describes the object as "extremely faint." The following ephemeris is given for Berlin midnight:—

	R.A.			Decl.	
	h	m	s	°	′
Nov. 1..	21	26	2	+ 7	3·4
5..	21	37	6	+ 8	37·5
9..	21	49	59	+10	19·3
13..	22	4	59	+12	12·1
Nov. 17..	22	22	14	+14	12·0
21..	22	41	54	+16	18·0
25..	23	4	6	+18	26·6
29..	23	28	48	+20	33·2

A bright Comet was discovered by Barnard on October 3, R. A. 112° 51′, N.P.D. = 117° 54′, which is now out of reach of northern observers, though southern may find the following ephemeris useful:—

	R.A.		Decl.	
	h	m	°	′
Nov. 11..	12	22·3	−56	26
23..	13	29·5	−55	4
Dec. 5..	14	17·7	−52	32
17..	14	52·6	−49	40

Wolf's periodic Comet is still bright throughout November, though it is moving south. Ephemeris:—

	R.A.			Decl.	
	h	m	s	°	′
Nov. 1..	4	40	51	−2	36·7
5..	4	39	39	−4	34·8
9..	4	37	57	−6	25·0
13..	4	35	50	−8	5·9
17..	4	33	26	−9	36·4
Nov. 21..	4	30	51	−10	55·5
25..	4	28	10	−12	2·9
26..	4	25	31	−12	58·5
Dec. 3..	4	22	58	−13	42·2

Winnecke's periodic Comet will be fairly bright in April next, but Dr. Haerdtl gives an extended ephemeris in the hope of its being picked up early; though its brightness is now only 0·01 of that on April 1.

	R.A.			Decl.			R.A.			Decl.	
	h	m	s	°	'		h	m	s	°	'
Nov. 1..	11	4	7	+15	2·6	Nov. 21..	11	29	22	+13	44·6
5..		9	13	14	45·3	25..		34	20	13	32·1
9..		14	18	14	28·7	29..		39	15	13	20·9
13..		19	21	14	13·0	Dec. 3..		44	7	13	11·3
17..		24	23	13	58·2						

Encke's Comet was observed on Oct. 4 at Göttingen, in R.A. $11^h\ 11^m\ 11^s$, Dec. +11° 15'·7, but no ephemeris is to hand for November.

RAINFALL IN 1891.—There is a general impression of there having been of late an excessive rainfall. We hear on many sides of floods and inundations, and in parts of the country during the present summer the rainfall appears to have been considerable. As regards the vicinity of London, it may tend to steady our thoughts if we give some particulars of the rainfall as recorded at the Royal Observatory, Greenwich.

1891.	Rainfall.	Average.	Departure from average.	Rainy days 1891.	Average.	Departure from average.
	in.	in.	in.			
January	1·563	1·989	−0·426	17	15	+ 2
February ...	0·054	1·484	−1·430	4	15	−11
March	2·143	1·461	+0·682	17	13	+ 4
April	0·718	1·661	−0·943	8	13	− 5
May	2·688	2·003	+0·685	18	11	+ 7
June............	0·962	2·022	−1·060	11	12	− 1
July............	3·388	2·470	+0·918	17	13	+ 4
August.........	3·715	2·350	+1·365	22	13	+ 9
September ...	0·819	2·251	−1·432	12	13	− 1
October	4·317	2·811	+1·506	20	15	+ 5

In the first quarter of the year there was a defect of 1·174 in., in the second quarter a defect of 1·318 in., and in the third quarter an excess of 0·851 in. In October the excess was 1·506 in., so that up to October 1891, the defect for the year was still 0·135 in. Amounts greater than 5 inches have been recorded in October in the years 1841, 1846, 1855, 1865, 1880 (=7·65 in.), and 1882. During the year 1891 there have been two daily falls exceeding 0·75 in., one on August 2 =0·886 in., and one on October 22 =0·774 in. The rainy days in 1891 have been 146, average =133. The impression of excessive rainfall may have been given from the circumstance that the rainfall was at times spread over a considerable number of hours. In February and April it will be noted that the days of rainfall were very few. The February rainfall was indeed phenomenal. W. E.

THE discovery of minor planets advances at such a rapid pace that it becomes difficult to keep proper account of them. Messrs. Palisa and Charlois seem to monopolize this particular branch, and since our last notice in the May number they have added 12 to the list, making a total of 321. Of the five discovered during September, four are placed to the credit of Charlois, who has an inferior instrument but a better sky than the Vienna observer. The supposed new planet discovered by Palisa on August 14 turns out to be identical with Medusa (149).

Minor planet No. 306 has been named Unitas, and 309 Fraternitas.

IN the 'Astronomical Journal' No. 246, Prof. Glasenapp gives the orbit of Σ 186. He makes a period of 150·8 years.

In the same number S. W. Burnham gives recent measures of the two double stars:

	Position-angle.	Distance.	Epoch.
β Delphini	331°·6	0″·38	1891·45
85 Pegasi, AB.	151 ·8	0 ·78	1891·56
AC.	354 ·7	24 ·58	1891·56

And J. M. Schæberle gives an ephemeris for 85 Pegasi to the end of present century.

WE regret to hear of the death of Franz Friedrich Ernst Brünnow at the age of nearly 70. Though his active astronomical career was cut short by ill-health some years ago, it was already a long and important one. He was appointed Director of the Bilk (near Düsseldorf) Observatory in 1847, and while there produced an important memoir on De Vico's comet, as well as a very successful text-book on spherical astronomy. From 1851 to 1854 Brünnow was at Berlin as First Assistant, but was called from there to the United States, where he was prominently connected with the observatories of Ann Arbor (Michigan) and Albany, and edited the 'Astronomical Notices.' In 1866 he was made Astronomer Royal for Ireland, and two volumes contain his work on Stellar parallax of this date; but he resigned this post in 1874 on account of overwork and weakness of the eyes. He died on 1891, August 20, of heart disease.

ALSO of Professor Ferrel, of the United States, one of the foremost meteorologists of our time. He paid great attention to the mechanics of the atmosphere, treating questions conected therewith by strictly scientific methods. Many of his papers are to be found in the U.S. official publications, and were not greatly known; but one of his later separate works, "A Popular Treatise on the Winds," has probably enjoyed a wider circulation. In it he treats of the general motions of the atmosphere, and of monsoons, cyclones, waterspouts, &c., and he gives therein the results of his investigations and conclusions on these and related subjects. The work forms a valuable addition to meteorological literature, and the loss of its author is keenly felt by the meteorologists of all countries.

Also of Dr. J. C. Lamp, on September 18, at Davos. Dr. Lamp commenced his astronomical life in Bothkamp, in the observatory of Herr von Bülow; but in 1888 he returned to Berlin, and was the most zealous of assistants to Dr. Auwers from that time until lung-trouble developed itself, and cut short a most promising career at the age of 34.

Congratulations to Mr. A. M. W. Downing on his appointment as Superintendent of the 'Nautical Almanac,' in succession to Dr. Hind, who will resign at the end of this year. Mr. Downing's contributions to Astronomy have generally dealt with large masses of figures, and his long term of service at the Royal Observatory, Greenwich, will be utilized to manifest advantage in his new sphere of activity. He is a Secretary of the Royal Astronomical Society, and was recently one of the Editors of this excellent magazine.

The Newall Telescope and dome are all but completely mounted on their new site at Cambridge, and Mr. H. F. Newall has built himself a house close by, whence he has been superintending the erection. Anyone who has had a similar experience knows the severity of such work, and will wish Mr. Newall a speedy and successful termination to his more fatiguing labours.

To Double-Star Observers.—Prof. S. W. Burnham is preparing a general catalogue of all stars discovered by himself, and would be glad to receive any unpublished measures of these stars, Nos. 1 to 1224.

An excellent article by the Rev. A. L. Cortie, S.J., on "Some recent Studies on the Solar Spectrum," which originally appeared in 'The Month,' has been reprinted, and will well repay anyone who takes the trouble to acquire and peruse a copy.

We have not yet heard of any steps being taken towards ing a successor to Mr. Pogson at the Madras Observatory.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 182. DECEMBER. 1891.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, November 13, 1891.

Lieut.-Gen. J. F. Tennant, C.I.E., R.E., F.R.S.,
President, in the Chair.

Secretaries: E. B. Knobel and A. M. W. Downing, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Downing. One hundred and ninety-one presents have been received since the last Meeting. Amongst those calling for special mention are:—J. E. Gore, 'Star Groups,' presented by the Author; T. Cooke & Sons, 'Adjustment and Testing of Telescopic Objectives,' presented by the Authors; F. W. Very, 'On the Distribution of the Moon's Heat, etc.' presented by the Author; 'Lick Observatory Reports of Observations of the Total Eclipse of the Sun, Dec. 21–22, 1889,' presented by the Lick Observatory; T. W. Backhouse, 'The Structure of the Sidereal Universe,' presented by the Author; L. Struve, 'Bestimmung des Mondhalbmessers,' presented by the Author; C. Flammarion, 'Copernic et la découverte du Système du Monde,' presented by the Author; Dr. W. Huggins, 'Address delivered at the Cardiff Meeting of the British Association 1891,' presented by the Author; Th. von Oppolzer 'Traité de la détermination des Orbites des Comètes et des Planètes, trad. par E. Pasquier,' presented by Mr. R. E. S. Cooper; J. A. C. Oudemans, 'Triangulation von Java,' Abth. iii., presented by Prof. Oudemans; J. K. Rees and H. Jacoby, 'Catalogue of Negatives of the Sun, Moon, and Stars, taken by Lewis M. Rutherfurd, 1858–78,' presented by the Authors; Photographs of Spectra of Solar Prominences taken by Prof. G. E. Hale, presented by Prof. G. E. Hale; Lick Observatory, Photograph of Jupiter and Photograph of the Cluster in Hercules, presented by the Lick Observatory;

Two drawings of Jupiter, presented by Mr. G. T. Gwilliam; Slater's Armillary Sphere, presented by Prof. Slater.

A vote of thanks was accorded to the donors of the presents.

Mr. Downing read an extract from Prof. Holden's letter which accompanied the photograph of the Cluster in Hercules, presented by the Lick Observatory:—

"1891, Sept. 28.

". . . I am sending a positive copy on glass of a negative taken July 28, 1891, by Prof. Campbell and myself with the great telescope. The copy is not as good as the negative, and the negative is not as good as it ought to be. Still you can see some very interesting features. The easiest to make out are dark channels in sets of 3 ⅄^{x} duplicating the 3 channels figured by Lord Rosse in the Phil. Trans. If we call the intersection of such channels x, there are 13 x's on our negative of Aug. 25, 1891. These have been verified visually, and some of them are shown very well on a beautiful negative of the Cluster which Mr. Roberts kindly sent us.

"The trifid nebula has channels of the same sort, as you remember. There are various other features worth noting, of which I will not speak now. To bring them out clearly we need an exposure of 4 hours on a night without wind. Our longest exposure has been $2^{h}\ 50^{m}$."

The President. I examined the photograph of Jupiter the other day, and was much impressed with the definition of important parts of the planet, and particularly with the extremely numerous details visible upon the belts.

Mr. Ranyard asked if it would be possible to show this beautiful photograph on the screen.

Mr. Knobel. We will endeavour to do so, but the photograph is so very granular that I fear the result will not be very satisfactory.

The photograph was projected later in the evening, and the details on the belts were well seen. The red spot was very sharp, as also was the disc of a satellite just after transit egress, the shadow of the satellite being still upon the disc.

Mr. Knobel read a paper by *Mr. E. E. Barnard*, entitled "Observations of the Spots and Markings on the Planet Jupiter, made with the 12-inch Equatorial of the Lick Observatory." The Planet has shown a remarkable amount of detail during the present opposition. The southern hemisphere exhibited the great red spot, which has regained much of its former distinctness in colour and form, and new red spots have been formed in addition to a large number of white spots. These latter markings are characteristic of the southern hemisphere, though individual white spots have been seen on the northern half. A number of small dark spots have been formed on the northern regions, and these have extraordinarily short periods of rotation. Observations have been made of the relative light and size of the four moons, and these will be continued. The fourth satellite seems to vary considerably in its light.

Detailed observations are given of a small oblong dusky spot in the southern hemisphere; of the small black spots in the northern hemisphere which formed during the present year in identically the same location as a similar group in 1880; of a new oblong red spot which formed near the great red spot in August 1890, the origin of which is probably connected with the great red spot; of the large white spots in the southern hemisphere, one of which skirted the red spot without any apparent change of latitude; of the long red spot in the southern hemisphere; of the great red spot, and of the phenomena of the satellites.

The great red spot seems to be stationary in longitude, its motion appearing to be the same as that assigned to Marth's System II. It still remains the slowest rotating object on the planet.

The first satellite has not been seen double since the observation on Sept. 8, 1890, and later observations of the phenomena rather discourage the idea of actual duplicity, although the conditions for reobservation will not be most favourable until 1892.

The satellite has been seen egg-shaped when in relief on the dark belt, both by Mr. Burnham and myself, and this cannot be due to any malformation of the satellite, since that object has always appeared perfectly round only a few minutes before the elongated phase, the transition being rather sudden. This particular phase and the apparent duplicity is explained by a bright belt on the satellite, and as the pointed end is sometimes preceding, and at other times following, Satellite I. must rotate on its axis in a time different to its period of revolution.

After observing the planet for some twelve years, I can scarcely advocate the theory that the visible surface of Jupiter is cloud surface. It would be more consistent with observed phenomena to suppose the surface to be in a plastic or pasty condition, the belts and markings being merely discolorations in this due to internal irruptions. The permanence and the changes of colour of the markings are against the cloud theory. One might combine the two theories, and account for the shortcomings of the plastic theory by supposing local clouds of steam at or near the surface. I do not know to what extent the small density of the planet may militate against this, but the theory seems certainly worthy of consideration. Perhaps this theory has been advanced before, as it seems a natural one; if so, I have not seen it.

Mr. Knobel read another paper by *Mr. Barnard* on "Colour Changes in the Markings on the Surface of the Planet Jupiter."

A careful study of the numerous details during 12 years has led to the discovery that the red colour of any marking is an indication of age. When a spot first appears it is dark, but after some time turns red. No deviation from this rule has been discovered. The black spots in 1880, and the similar spots in 1890, soon turned red. The new red spot, which exhibited no trace of red in August 1890, is now the most conspicuous object on the planet and is of a deep red colour. Inquiries into the history of the great red spot show that in the early observations in 1872 by Mr. Corder ('Observatory,' vol. v. page 21) "its colour was not noted as red," and observations by Dr. Terby confirm this. From a knowledge of the time occupied by the dark phase of the spots, I think it would be safe to say that the great red spot was not many months old when seen by Mr. Corder and M. Terby in 1872.

This rule (of colour change) seems to apply also to the equatorial belt, the dark portions being new or recent formations.

Mr. Knobel read a paper "On the supposed Duplicity of the First Satellite of Jupiter," by *A. Stanley Williams.*

With reference to Mr. Barnard's observation of the apparent duplicity of Satellite I. on Sept. 8, 1890, I would ask whether the observed phenomena may not have been due to the satellite being seen in transit as a dark spot close to a dark spot on the surface of Jupiter. Mr. Barnard's observation was interrupted, and it was not possible to follow the satellite closely owing to the presence of visitors, or the difference in rate of motion could not in such case have escaped being noticed. Mr. Barnard's explanation of his observation appears to me inadequate, since it would require the satellite to be much larger than it actually is. On Sept. 5, 1890, three days before Mr. Barnard's observation, the accompanying sketch-map was made with a 6½-inch Calver reflector, powers 150 and 225, of the equatorial regions of the planet between $8^h\ 14^m$ and $11^h\ 27^m$ G.M.T. The little dark spot marked "*a*" was in mid-transit at $8^h\ 42^m$, and therefore followed the zero meridian of System I. of Mr. Marth's Ephemeris ('Monthly Notices,' vol. 50, p. 346) by $3^h\ 30^m{\cdot}7$. The motions of the markings last year agreed closely with the ephemeris, and a few days would make no appreciable difference. The dark spot would transit therefore at $15^h\ 20^m{\cdot}7$ G.M.T. on Sept. 8th, and, according to the Nautical Almanac, the Satellite was in mid-transit on that night at $15^h\ 27^m$. It would therefore be seen projected on the bright surface of the equatorial zone just north of the spot "*a*," and the appearance of the two would be exactly that represented by Mr. Barnard in fig. 4, plate 14.

Dr. Spitta. Several years ago I made a number of experiments on the question of the transits of Jupiter's Satellites, and I feel convinced that most of the phenomena given by our American *confrères* are due entirely to contrast. Since the announcement by Mr. Barnard of the probable duplicity of the first satellite and of the egg-shaped appearance it sometimes assumes during transit, I have made further experiments which confirm the results I previously obtained. If you take an ordinary small white disk and allow it to transit a larger one, using the lime-light to illuminate both, then if the relative degrees of brightness of the disks are about equal to those of Jupiter and his first satellite, the ordinary phenomena of transit are produced. With a bright belt on the disk representing the satellite the same effects are seen, when it transits a bright belt of the disk representing Jupiter. If this disk is transferred, however, to a dark region on "Jupiter," then the elongated form is produced; and I have thus artificially produced the egg-shaped form seen by Mr. Barnard with the planet and its satellite. It must be remembered that irradiation makes the bright belt appear larger than the darker portions, and I think my experiments place the explanation of this egg-shaped appearance beyond dispute.

Mr. Knobel. Mr. Barnard states in his paper that the Satellite I. now transits the southern dark equatorial belt of Jupiter, and therefore cannot be easily seen as a dusky spot during transit. There was a transit of this satellite on Saturday last, which I observed, and when the satellite entered on the disk it appeared perfectly bright. After it had passed on to the disk about a quarter of the apparent diameter of Jupiter, I could see it as a dusky spot and not as a bright one. The satellite was clearly seen on the southern belt. It is possible, therefore, at the present time to observe the transits of the satellite, even although the dark spot may not be so pronounced as when observed by Mr. Barnard.

Capt. Noble. I was struck by two remarks in Mr. Barnard's papers. The first was in connection with what he says about the probable plastic condition of the superficial markings which we see on Jupiter. He points out that the red spot has been the slowest in rotation of any object on the disk.

Mr. Stanley Williams, if I remember rightly, watching the planet, saw a white spot advancing towards the red spot, and he was interested to ascertain whether this white spot would go above or below the red one. It went below it and reappeared on the other side. It has struck me, then, whether the red spot may not be a protuberance of what we may call the substance of Jupiter himself; in fact, that it may be a portion of the red-hot material of the planet protruding through the plastic superficial visible surroundings of Jupiter.

Mr. Ranyard. The difference in periods of rotation of Jupiter indicated by different markings on the planet has, I think, been fully proved previous to this paper by Mr. Barnard; his observations confirm what was previously known. Of itself this is sufficient evidence that we do not see the surface of Jupiter, but that the bright spots and dark markings we see are in a gaseous envelope. Some years ago I put together, in a paper published in the 'Monthly Notices,' all the observations of occultations of stars by Jupiter, and I think showed that there was ample evidence that stars and satellites, when occulted by the limb of Jupiter, do not disappear suddenly, but can be seen apparently projected on the limb of the planet—that is, really through its atmosphere—for several minutes after contact. A 7th-magnitude star was thus seen by Prof. Pickering through at least a depth of 1000 miles of

Jupiter's atmosphere, and its light disappeared and appeared again several times. The markings we see must therefore be in a deep gaseous envelope. This fact and the different periods of rotation are either of them alone sufficient to disprove the plastic theory. There seems to be strong evidence of a connection between sun-spot activity and the markings on Jupiter, and it is interesting to hear that there is now rapid change taking place in the bright and dark markings on Jupiter with the increase of sun-spot activity *.

Mr. Turner. As one whose work is in an entirely different direction from the study of planetary detail, I should like to express the satisfaction one feels to know that we have observers like Mr. Williams and Mr. Denning in England who are capable of entering into friendly rivalry with Mr. Barnard and other American observers.

Mr. Howlett. How long could a double satellite exist with components revolving in such close proximity as the double suggested by Mr. Barnard without drawing together and forming one satellite?

Mr. Ranyard. What would be the pressure of two such masses 1000 miles in diameter? At the point where they touched their mutual gravity would crush them together, even if made of iron.

The President. I am afraid the last questions must remain for the present unanswered, for there is nobody present to give a dogmatic response. In asking you to accord your thanks to Mr. Barnard and Mr. Williams, I would point out that those who take the trouble to go into these questions of planetary detail are doing very useful service to astronomy, although perhaps it is not a very pretentious one.

Mr. Turner read a paper on "New Forms of Levels" by *Dr. A. A. Common* and himself.

The question of new forms of levels may seem a very small matter, but it is for purposes of accurate astronomy a very important one. The great difficulty about the ordinary form of level is the want of uniformity of the scale. On the occasion of the recent determination of the longitude Paris—Greenwich, we found it necessary to spend a great deal of time in examining this point. Equal divisions on the glass cylinder of the level do not correspond to equal differences of inclination. I was led, therefore, to devise a new form of level in which the bubble is always brought to the same part of the tube by means of a micrometer-screw that can be used to raise or lower one end of the glass tube. The amount of rotation of the screw to bring the bubble central is read off as in the ordinary micrometer, and this replaces the scale reading of the ordinary form.

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* In 1870 I wrote a paper in the 'Monthly Notices' drawing attention to several coincidences previous to that date between periods of sun-spot activity and periods of rapid change upon Jupiter. Since that date we have had rapid change taking place from 1880 to 1883 with great sun-spot activity, and now again there seems to be an increase of activity on both the Sun and Jupiter.

Astronomers are familiar with the investigation of errors of a screw, and the errors of the level screw can be more accurately ascertained than the errors of scale reading of a level. Certainly the error of the screw would not be greater than the present scale error, and would probably be less. The object of the screw was not so much to reduce the accidental error of a reading as the systematic error. The new level would render it necessary (1) That in future four readings should be taken instead of two; this meant a longer operation, but the increased systematic accuracy would more than counterbalance the additional time. (2) The time spent in the calibration of the scale would be no longer necessary. (3) In this form of level the part of the scale which was used might be selected at will, and the best part of the tube always used. (4) There was always a tendency to stop short of the true position in the ordinary form of level, but in the present form errors due to this tendency could be determined by bringing up the bubble in opposite directions with the screw. (5) There were some cases in which levels were fixed to the instrument, as in the case of an altazimuth at Greenwich, and were supposed to be always in adjustment. After standing for several days, and particularly after rapid changes in the weather, these levels were often seriously out of adjustment, so that the bubble was off the scale. This led to inconvenient delay; whereas if the new levels were used, and adjustment were made part of the ordinary every-day use of the instrument, no inconvenience would occur.

The second part of the paper was due to Dr. Common, who suggested that instead of the ordinary bubble with its variable length and other inconveniences, an entirely new form of level should be made. It was really a modification of the Bohnenberger eyepiece, and consisted of a small horizontal telescope mounted on legs as in the striding level, with a reflecting prism attached to the object-glass, so as to reflect light passing from the eye-end downwards on to a surface of mercury, from which the light would be reflected back along the tube. By placing illuminated wires at the focus of the object-glass, the light from the wires would be reflected back to the same plane as the wires. A micrometer screw would enable the wires and their image to be made exactly coincident.

The instrument was illustrated by a sketch on the blackboard.

Mr. Turner said he felt that any such additional method of determining level, which was an independent one, was very valuable.

Mr. Brett. How do you know that you have got a true cylinder to your level—the cylinder, I mean, in which the liquid moves? You have some method of determining that I presume? If the cylinder is not regular in shape, the size and shape of your bubble will vary, and there is no use in measuring it.

Mr. Turner. That is the difficulty with the ordinary form. The bubble is not always the same size owing to changes of temperature, or the same shape owing to errors of form in the cylinder. With the new form of level you are not concerned with different parts of the tube, for as the bubble is always brought to the same portion of the tube for the two readings in reversed positions, any variations in the tube have no effect whatever.

Mr. Lecky. If there was a speedy method of getting the centre of a bubble by a proportional compass or otherwise, then many of the difficulties of the present form would disappear, and instead of a mark at each end of the level one fiducial point could be marked on the centre of the glass, to which the centre of the bubble could be brought with more accuracy than the ends of the bubble to the two fixed marks. I have often throught that the "Circular floating collimator" invented by the late Captain Kater deserved more notice than it has received as an instrument for use with the Transit instrument, for by reversion it combines both level and collimator. Many years since I had one in use, made by a friend of mine, in the South of Ireland.

Mr. Plummer. The difficulty, I think, is that the bubble under different temperatures is of different lengths. There should be some sort of chamber, so that when or before the micrometer is turned the liquid can be compressed or released so as to get the same length of the air-bubble under all circumstances.

The President. I think the subject which Mr. Turner has taken up is a very old one. I recollect looking into the subject when I was in the constant habit of using levels. I have no doubt that Mr. Turner's idea will give great facility and greater accuracy in reading levels. There are other things, however, that we can do. The exact mode of making levels used to-day is described, I think you will find, in Pearson's 'Astronomy,' and we have not advanced since that time. You will find the present system of making levels is about as unsatisfactory as possible, especially the manner of obtaining the uniform scale. There is no security whatever that the level shall be a good one, except the tests with the bubble-trier. When I was a member of the Trigonometrical Survey of India, I tried, amongst other things, to devise a better level than we have. I abandoned the cylindrical tube altogether, and used a circular curve cut out of one side of a solid prism of glass. This I expected would be more sensitive than the present form, but I never was able to carry it out. [Black-board illustration.]

At present we use a very light and volatile fluid in levels, but this is not essential. In 1851 Prof. Piazzi Smyth exhibited some levels in which he used heavier fluids, and he found these more sensitive in every way; he also used a supplementary chamber as suggested by Mr. Plummer. The important thing is to get a fluid in the level of considerable density and mobility, and if you make your tube of the particular form shown in the black-board diagram, you would probably be able to use mercury, which would have a great number of advantages over the levels filled with ether now in use. Mr. Turner's suggestion is, I believe, a good one, but we can do something to improve the curve of the level and to alter the liquid; certainly something should be done to alter the very unsatisfactory state of levels.

Mr. Brett. Why not use chloroform as the liquid? It is extremely heavy and extremely mobile, and would be a much better liquid to use.

A vote of thanks was passed to Mr. Turner and Dr. Common for their paper.

Dr. Spitta read "A note on some Photometric Experiments connected with the Application of the Law of limiting Apertures to Small Object-Glasses." In some experiments connected with the paper read before the Society in November last, certain doubtful and equivocal results immediately followed the application of a diaphragm to the telescope in use, and these were of so pronounced a character that the use of such means for reducing the intensity of the luminant had to be abandoned. A doubt had always existed as to whether it was scientifically accurate or strictly philosophical to apply the law of limiting apertures to an object-glass, considering its compound nature and different modes of manufacture. The accompanying table gives the results obtained from photometric tests of objectives by different makers, English and foreign. The first column gives the distinguishing name of the O.G. examined; the second and third its aperture and focal length; the fourth and fifth the actual ratios photometrically obtained when the aperture was lineally reduced to one half and one quarter respectively.

Table of Ratios Photometrically obtained.

	ins.	ins.		
I. Alvan Clark......	6·5	93	$1 : \frac{1}{3{\cdot}35}$	$1 : \frac{1}{14{\cdot}84}$
II. Tulley	3·1875	46·5	$1 : \frac{1}{3{\cdot}3}$	$1 : \frac{1}{7{\cdot}5}$
III. Clerkenwell	2·625	42	$1 : \frac{1}{2{\cdot}63}$	$1 : \frac{1}{11{\cdot}70}$
IV. Watson	2·8125	41	$1 : \frac{1}{2{\cdot}11}$	$1 : \frac{1}{15{\cdot}91}$
V. Grubb	2·875	40·8	$1 : \frac{1}{2{\cdot}31}$	$1 : \frac{1}{5{\cdot}07}$
VI. A. and N........	2·375	24·75	$1 : \frac{1}{2{\cdot}47}$	$1 : \frac{1}{8{\cdot}23}$

These results were also embodied in curves exhibited by means of the lantern.

An examination of the table and the curves shows that in all cases, except perhaps the Alvan Clark, the rays passing through the outer zones do not contribute to the intensity of the focal image as much as theoretically they should. It would seem that focal length is not a factor of importance, but I submit that a solution of the difficulty very probably lies in the aplanatisation of the respective glasses. Perfection of definition is sometimes effected, unconseriously it may be, by pushing the focus of the rays from the outer zones aside, so as to be finally lost or cut off by suitable diaphragms. This conclusion was forced on me by an examination of one of the telescopes examined photometrically with diverging light, which of course exaggerated the defect, when I found, with all internal diaphragms removed, that the outermost zone had a focus sensibly different in length to those from neighbouring and central ones. Yet this object-glass in the tube had good defining power, going in and out of focus with slight movement of the eye-piece.

These experiments offer a suggestion how it came about that Dr. V. L. Charlier found that diaphragms did not control the photographic intensity of light according to theoretical considerations. They also render it easy to understand why half the linear aperture of a large telescope has so often been noticed to reveal fainter stars than the full aperture of another of the same linear dimensions.

The errors are not due to differences in angles of incidence of the cylindrical beam upon different zones, for the Clerkenwell glass reversed, so that its sensibly flat posterior surface received the rays, gave results similar to those first obtained.

Dr. Müller, of Potsdam, working with a Zöllner photometer, reduced his aperture in such ratios that the computed values of the logs of brilliancy of a star were 1·0742, 0·7059, and 0·4437, but the observed ratios were 0·3695, 0·3439, and 0·2687. He found, after the aperture had been reduced one-half, the observed and computed ratios had a tendency to agree. With a larger telescope (*i. e.* one of 135 mm. aperture) the differences between observed and computed ratios became almost *nil*.

Wolff, after experiments with an object-glass of 37 mm. diameter, comes to much the same conclusion, although, as a matter of fact, he did not push his investigations any further with the reduction of aperture to telescopes of larger dimensions.

The Astronomer Royal. I should like to ask Dr. Spitta whether he thinks the small aperture of the object-glasses photometrically tested has anything to do with the imperfections he has noticed. The question is whether these small object-glasses he has used have been as carefully made as larger ones are, and, therefore, whether his results are altogether conclusive. We know that small object-glasses are in many cases meant to be used as finders, and opticians do not take so much trouble in figuring them as they do with larger object-glasses for observing purposes. The first on Dr. Spitta's table is of 6½ inches diameter, and that is, I think, a fair aperture for testing; but when we come to 2½ inches, or something like that, I should very much doubt whether sufficient care has been taken in figuring these small object-glasses to enable us to profitably apply these crucial tests to them. I would also ask what were the apertures of the object-glasses Dr. Müller has examined.

Dr. Spitta. With regard to what the Astronomer Royal has said, I would say that the Tulley object-glass was specially made, and is supposed to be of the first class. He was specially commissioned to make that telescope, and it was used years ago by no less authority than Prof. Pritchard for eclipse observations. The focus is remarkably fine, but the light-ratio is very poor.

Mr. Plummer. As to the small object-glasses, I think the comparison given by Dr. Müller is very instructive, because those figures were obtained by testing the object-glasses of the Zöllner photometers. The diameter of these object-glasses is small, about 2½ inches, I think, very comparable with those measured or examined by Dr. Spitta. Dr. Müller says that when he applied photometric observations to the intensity of the images formed in larger telescopes, and compared them with results obtained by limiting the aperture of the same telescope, he found that the agreement of the results was practically perfect.

Does Dr. Spitta consider the alteration in size of the diffraction image, when the aperture is diminished, would affect the results given in his diagram?

Dr. Spitta. I did take that into consideration, and for that reason I used the compound wedge in these experiments, thus getting over that difficulty entirely.

A vote of thanks was passed to Dr. Spitta for his paper.

Mr. F. McClean read a paper on "Comparative Photographic Spectra of the Sun and Metals.—Series I. and II."

I wish to lay before the Society two series of photographic spectra, in which the spectra of the Sun and of fifteen metals enumerated are mounted in parallel sections in order to facilitate comparison and identification of the lines. Series I. contains spectra of the Sun, iron, platinum, iridium, osmium, palladium, rhodium, ruthenium, gold, and silver. The last eight constitute the platinum group of metals. Series II. contains the spectra of the Sun, iron, manganese, cobalt, nickel, chromium, aluminium, and copper. These seven metals constitute the iron-copper group. Each series consists of six sections, corresponding in scale to Angström's charts. The air-spectrum is always present when an electric spark is passed between metallic electrodes in air, but a special series of photographs are requisite to obtain it at its best. It is also necessary to eliminate lines due to impurities in the specimens of metals employed as electrodes. Iron, calcium, and barium are the chief impurities. The fluting attributed by Thalen to aluminium oxide is shown in the aluminium spectrum. The spectra of iridium, ruthenium, and rhodium are, I believe, practically new work, and so, I think, are some of the other spectra of Sections I. and II. Identification of iron, chromium, manganese, cobalt, and nickel can be readily made, but it is impossible to get complete results until impurities are eliminated and a double set of photographs obtained. The spectra extend from wave-length

3800 to wave-length 5750 tenth-metres, or from H to near D. The mounted photographs are reproduced by the Direct Photo-Engraving Company, and the white lines unfortunately become a little coarser in the process, but I hope the photographs may be of some service to others engaged in spectrography.

Mr. Ranyard. During a visit to Mr. McClean, I was very much struck with the remarkable care he is giving to this very difficult research. The photographic and electrical difficulties are great. The photographs are beautifully sharp, the scale is the same as that of Angström, and the mounting is most accurate. The common lines in the spectra are seen in exactly the same straight line down the whole series, and there is really beautiful definition in many of the spectra.

The President. I am afraid there is hardly anybody here who can comment with much advantage upon Mr. McClean's work. The fact is that it requires an enormous amount of care and attention to turn out work of this kind, and when Mr. McClean gives us the full benefit of it, we can only feel grateful, and cannot criticise. We are very thankful to Mr. McClean for the very great service he has performed.

Dr. Common read a paper entitled "Note on some Photographs of Jupiter taken with the 5-foot Reflecting Telescope." The paper is not intended to be a detailed description of the photographs, but was brought forward to tell the Society of a new mirror that has been made for the five-foot telescope, and of the experiments made with the new mirror since July with the object of testing the light-grasping power and the defining power. For this preliminary work Jupiter was very conveniently situated, and has been photographed directly at the focus, and also by the use of various enlarging lenses attached to a special enlarging-apparatus. The images vary from about $\frac{1}{13}$th of an inch in diameter, that being the size without enlargement, up to an inch and a half, the greatest enlargement yet attempted. The results, speaking generally, have been very satisfactory.

The shortest possible exposure (obtained by hand-rotation of a circular shutter in front of the plate) gave a very good image of Jupiter at the direct focus. The exposure was certainly less than $\frac{1}{10}$th of a second, and yet the bells and other markings on the planet were easily visible with a lens. Extending the exposure to 2 seconds "burnt out" the detail on the planet, but showed all the satellites with sufficient density to be easily recognized, and their angular distance from the centre of Jupiter could be easily measured from the photographs. Enlarging the planet to half an inch in diameter increased the necessary exposure to from 3 to 6 seconds. Larger sizes were obtained by the use of other enlarging-lenses. After making a series of photographs of the planet, I was very much surprised to find that with the planet $\frac{8}{10}$ths of an inch in diameter, and an exposure of 8 seconds, not only were details on the belts shown, but a satellite in transit could be easily recognized, and the shadow also made out. Satellites off the disc, but close to it, were also distinctly shown with round well-defined discs.

A series of photographs of Jupiter were shown by the lantern and the details in each photograph pointed out by Dr. Common.

A vote of thanks was accorded for the paper.

The following papers were announced:—

T. W. Backhouse. "Note on S. W. Burnham's invisible Double Stars."

Lieut.-Gen. Tennant. "On the Orbit of Spitaler's Comet (VII. 1890)."

E. E. Barnard. "Colour-changes in the Markings on the Surface of the Planet Jupiter."

E. E. Barnard. "Observations of the Spots and Markings on the Planet Jupiter, made with the 12-in. Equatoreal of the Lick Observatory."

E. W. Brown. "On the Determination of a certain Class of Inequalities in the Moon's Motion."

S. W. Burnham. "Measures of Planetary Nebulæ made with the 36-inch Equatoreal of the Lick Observatory."

S. W. Burnham. "Corrections and Additions to the Observations of Sirius."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the year 1890, deduced from photographs taken at Greenwich, at Dehra Dûn (India), and in Mauritius."

Royal Observatory, Greenwich. "Observations of Wolf's Periodical Comet (*b* 1891)."

H. H. Turner and A. A. Common. "On new Forms of Levels."

A. Stanley Williams. "On the supposed Duplicity of the First Satellite of Jupiter."

W. H. Maw. "Double-Star Measures 1888–91."

H. Jacoby. "On the Reduction of Transit Observations by the method of Least Squares."

Rev. A. Freeman. "Reappearance of Saturn's Ring."

E. J. Spitta. "Note on some Photometric Experiments connected with the application of the Law of Limiting Apertures to Small Object-glasses."

F. McClean. "Comparative Photographic Spectra of the Sun and the Metals: Series I. & II."

A. Marth. "Ephemerides of the Satellites of Saturn, 1891–92."

A. A. Common. "Note on some Photographs of Jupiter taken with the 5-foot Reflecting Telescope."

Prof. A. Cayley. "Note on the Lunar Theory."

Oliver J. Lodge, D.Sc., F.R.S., Professor of Physics, University College, Liverpool, was duly elected a Fellow of the Society.

The following Candidates were proposed for election as Fellows of the Society :—

E. H. Lees, Orbost, Victoria, Australia (proposed by R. L. J. Ellery); *Prof. J. S. Slater*, Civil Engineering College, Subpore, India (proposed by A. M. W. Downing); *J. de Mendizábal Tamborrel*, 13 Calle de Jesús, Mexico (proposed by E. B. Knobel); *Arthur Thornton, M.A.*, Mathematical and Science Master, Giggleswick School, Yorks. (proposed by E. W. Brown); *W. Livingstone Watson*, Ayton, Abernethy, Ayrshire (proposed by Dr. R. Copeland).

MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

The first meeting of the present Session was held on Wednesday evening, the 18th November, at the Institution of Civil Engineers: Mr. Baldwin Latham, M.Inst.C.E., President, in the Chair.

Eleven new Fellows were elected.

Mr. R. H. Scott, F.R.S., gave an account of the proceedings of the International Meteorological Conference, which was held at Munich, from August 26th to September 2nd.

The following papers were also read:—

1. "Account of an Electric Self-recording Rain-gauge," by Mr. W. J. E. Binnie, B.A. This is a very ingenious instrument, and has been constructed on the assumption that all drops falling from an orifice or tube are identical in weight, so long as the dimensions of the orifice are not varied.

2. "On Wet and Dry Bulb Formulæ," by Prof. J. D. Everett, F.R.S. This is a criticism of the methods investigated some years ago by M. August and Dr. Apjohn for determining, by calculation, the maximum vapour-tension for the dew-point from the temperatures of the dry and wet bulb. Prof. Everett also criticises the values adopted by Regnault, and says that in presence of the uncertainty as to a rational formula, he thinks Mr. Glaisher did wisely in constructing his table of factors, which give the dew-point approximately by the most direct calculation which is admissible. The inherent difficulties of hygrometric observation and deduction are great, and have not yet been fully overcome.

3. "Results of Meteorological Observations made at Akassa, Niger Territories, May 1889 to December 1890," by Mr. F. Russell, F.R.G.S. This is in continuation of a former communication respecting the same place. After detailing the results of the various observations, the author says that this period was very unhealthy, and the year 1890 especially so. The weather was exceptionally dry, with small-pox and phthisis amongst the native population. The West Coast reports generally were also unfavourable in reference to the condition of resident Europeans, and at the principal ports quarantine regulations were put in force consequent upon an outbreak of yellow fever in places situated to the south-west. At Bonny ten deaths occurred from November to February out of a population of some sixteen Europeans.

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE first ordinary Meeting of the second Session was held on Nov. 25 at Barnard's Inn Hall, Holborn, the President, Capt. W. Noble, F.R.A.S., in the Chair.

Mr. Elger gave a *précis* of the report of the Lunar Section which he presented to the Association, and which will be published shortly. He also read papers on the recent lunar eclipse by Miss Everett, Mr. Gore, and Mr. Radmore. *The President* and the *Rev. A. Freeman* both described their observations, but differed much as to the colour of the eclipsed Moon, the former describing it as reddish green, the latter as bluish grey. *Mr. Ranyard* remarked on the difference of the appearance of the Moon in different eclipses; *Mr. Maunder* referred to Dr. Boeddicker's recently published observations as showing that the Earth's atmosphere reached a height of 190 miles. *Mr. Downing* pressed the importance of observing the times of passage of the shadow over the different formations, which *Mr. Elger* thought could only be observed to the nearest minute, but the *Rev. A. Freeman* to the nearest five seconds. *Mr. Ranyard* and *Mr. Kennedy* referred to Prof. Eastman's photographs of a lunar eclipse, and to the difference between the shadow as seen by the eye and as shown on the photographs.

Mr. Elger read a paper by Lt. Molesworth on a method of ascertaining whether the Moon had an atmosphere, concerning which the *President* stated that his disbelief in the existence of such an atmosphere had been greatly increased by an observation of an occultation of Saturn.

Rev. A. Freeman read a paper on the transit of the shadow of Titan, and pointed out that the shadow of Rhea would transit on the morning of Dec. 2. *Mr. Cottam* had observed three such transits of Titan's shadow in 1861 with a 2¾-inch telescope.

Mr. Schooling exhibited and described his apparatus for astronomical photography, and especially recommended the use of short-focus portrait-lenses for photographing nebulæ, and expressed his hope that a Photographic Section would soon be set on foot. *Mr. Seabroke* spoke of his own experiments in celestial photography, and *Mr. Kennedy* advised placing the plate at an angle of 45° as a speedy method of ascertaining the focus, whilst *Mr. Petrie* urged the necessity of the Association forming a collection of lantern-slides.

Mr. Holmes read a paper on the proportion of light transmitted by refracting telescopes, which he said was much less than was commonly supposed, whilst the efficiency of a good reflector was greater than many imagined. A good 12-inch reflector was about equal to a 12-inch refractor.

Twenty new Members were elected, and five candidates were proposed for election at the next Meeting.

An Astronomer's Work in a Modern Observatory.

[Continued from p. 375.]

I PASS now to another recent result that is of great cosmical interest.

The Cape photographic star charting of the southern hemisphere has been already referred to. In comparing the existing eye estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Professor Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky, that is in the Milky Way, the stars are systematically photographically brighter by comparison with the eye observations than they are in the poorer part of the sky, and that not by any doubtful amount but by half or three-fourths of a magnitude. One of two things was certain, either that the eye observations were wrong or that the stars of the Milky Way are bluer or whiter than other stars. But Professor Pickering, of Cambridge, America, has lately been making a complete photographic review of the heavens, and by placing a prism in front of the telescope he has made pictures of the whole sky like this. [Here two examples of the plates of Pickering's spectroscopic Durchmusterung were exhibited on the screen.] He has discussed the various types of the spectra of the brighter stars, as thus revealed, according to their distribution in the sky. He finds thus that the stars of the Sirius type occur chiefly in the Milky Way, whilst stars of other types are fairly divided over the sky.

Now stars of the Sirius type are very white stars, very rich relative to other stars in the rays which act most strongly on a photographic plate. Here, then, is the explanation of the results of our photographic star-charting, and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore we may come to the very remarkable conclusion that the Milky Way is a thing apart, and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch from the rest of the sidereal universe.

Here is another interesting cosmical revelation which we owe to photography.

You all know the beautiful constellation Orion, and many in this theatre have before seen the photograph of the nebula which is now on the screen, taken by Mr. Roberts.

Here is another photograph of the same object taken with a much longer exposure. You see how over-exposed, in fact burnt out, the brightest part of the picture is, and yet what a wonderful development of faint additional nebulous matter is revealed.

But I do not think that many persons in this room have seen *this* picture, and probably very few have any idea what it represents. It is from the original negative taken by Professor Pickering, with a small photographic lens of short focus, after six hours' exposure in the clear air of the Andes, 10,000 feet above sea-level.

The field embraces the three well-known stars in the belt of Orion on the one haud, and β Orionis (Rigel) on the other. You can hardly recognize these great white patches as stars; their ill-defined character is simply the result of excessive over-exposure. But mark the wonders which this long exposure with a lens of high intrinsic brilliancy of image has revealed. Here is the great nebula, of course terribly over-exposed, but note its wonderful fainter ramifications. See how the whole area is more or less nebulous, and surrounded as it were with a ring fence of nebulous matter. This nebulosity shows a special concentration about β Orionis..

Well, when Professor Pickering got this wonderful picture, knowing that I was occupied with investigations on the distances of the fixed stars, he wrote to ask whether I had made any observations to determine the distance of β Orionis, as it would be of great interest to know from independent evidence whether this very bright star was really near to us or not. It so happens that the observations were made, and their definite reduction has shown that β Orionis is really at the same distance from us as are the faint comparison stars. β Orionis is, therefore, probably part and parcel of an enormous system in an advanced but incomplete state of stellar evolution, and that what we have seen in this wonderful picture is all a part of that system.

I should explain what I mean by an elementary or by an advanced state of stellar evolution. There is but one theory of celestial evolution which has so far survived the test of time and comparison with observed facts, viz., the nebular hypothesis of Laplace. Laplace supposed that the Sun was originally a huge gaseous or nebulous mass of a diameter far greater than the orbit of Neptune. I say *originally*, do not misunderstand me. We have finite minds; we can imagine a condition of things which might be supposed to occur at any particular instant of time however remote, and at any particular distance of space however great, and we may frame a theory beginning at another time still more remote, and so on. But we can never imagine a theory beginning at an infinite distance of time or at an infinitely distant point in space. Thus, in any theory which man with his finite mind can devise, when we talk of *originally* we simply mean at or during the time considered in our theory.

Now, Laplace's theory begins at a time, millions on millions of years ago, when the Sun had so far disentangled itself from chaos, and its component gaseous particles had by mutual attraction so far coalesced as to form an enormous gaseous ball, far greater in diameter than the orbit of the remotest planet of our present system. The central part of this ball was certainly much more condensed than the rest, and the whole ball revolved. There is nothing improbable in this hypothesis. If gaseous matter came together from different parts of space such coalition would unquestionably occur, and, as in the meeting of opposite streams of water or of opposite currents of wind, vortices would be created and revolution about an axis set up such as we are familiar with in the case of whirlpools or cyclones. The resultant would be rotation of the whole globular gaseous mass about an axis.

Now this gaseous globe begins to cool, and as it cools it necessarily contracts. Then follows a necessary result of contraction, viz., the rotation becomes more rapid. This is a well-known fact in dynamics, about which there is no doubt. Thus, the cooling and the contracting go on, and simultaneously the velocity of rotation becomes greater and greater. At last the time arrives when, for the outside particles, the velocity of rotation becomes such that the centrifugal force is greater than the attractive force, and so the outside particles break off and form a ring. Then, as the processes of cooling and contraction proceed still further, another ring is formed, and so on, till we have finally a succession of rings and a condensed central ball. If from any cause the cooling of any of these rings does not go on uniformly,or if some of the gaseous matter of the ring is more easily liquified than others, then probably a single nucleus of liquid matter will be formed in that ring, and this nucleus will finally by attraction absorb the whole of the matter of which the ring is composed—at first as a gaseous ball with a condensed nucleus, and this will finally solidify into a planet. Or, meanwhile, this yet unformed planet may repeat the history of its parent sun. By contraction, and consequent acceleration of its rotation, it may throw off one or more rings, which in like manner condense into satellites like our Moon, or those of Jupiter, Saturn, Uranus, or Neptune. Such, very briefly outlined, is the celebrated nebular hypothesis of Laplace. No one can positively say that the hypothesis is true, still less can any one say that it is untrue. Time does not permit me to enter into the very strong proofs which Laplace urged in favour of its acceptance.

But I beg you for one moment to cast your imagination back to a period of time long antecedent to that when our Sun had begun to disentangle itself from chaos, and when the fleecy clouds of cosmic stuff had but commenced to rush together. What should we see in such a case were there a true basis for the theory of Laplace? Certainly, in the first place, we should have a huge whirlpool or cyclone of cosmic gaseous stuff, the formation of rings, and the condensation of these rings into gaseous globes.

Remembering this, look now on this wonderful photograph of the nebula in Andromeda, made by Mr. Roberts. In the largest telescopes this nebula appears simply as an oval patch of nearly uniform light, with a few dark canals through it, but no idea of its true form can be obtained, no trace can be found of the significant story which this photograph tells. It is a picture that no human eye unaided by photography has ever seen. It is a true picture drawn without the intervention of the hand of fallible man, and uninfluenced by his bias or imagination. Have we not here, so at least it seems to me, a picture of a very early stage in the evolution of a star-cluster or sun-system—a phase in the history of another star-system similar to that which once occurred in our own—millions and millions of years ago—when our Earth, nay, even our Sun itself, "was without form and void," and "darkness was on the face of the deep"?

During this lecture I have been able to trace but very imperfectly the bare outlines of an astronomer's work in a modern observatory, and to give you a very few of its latest results—results which do not come by chance, but by hard labour, and to men who have patience to face dull daily routine for the love of science—to men who realize the imperfections of their methods and are constantly on the alert to improve them.

The mills of the astronomer grind slowly, and he must be infinitely careful and watchful if he would have them, like the mills of God, to grind exceedingly small.

I think he may well take for his motto these beautiful lines:—

"Like the star
Which shines afar,
Without haste,
Without rest,
Let each man wheel
With steady sway,
Round the task
Which rules the day.
And do his best."

DAVID GILL.

The New Binary Star, β 6480.

B.A.C. 6480 is a naked-eye star, generally classed as sixth magnitude, near γ Lyræ. It precedes the latter $1^{m}\ 57^{s}$, and is 14′ north. In the course of the observations of the 12″ companion to γ Lyræ (OΣ 544) in June 1878, with the Chicago 18½-inch refractor, I found that the neighbouring star was a close and unequal pair, and at that time a difficult object with that instrument. It was measured on two nights, and again in the following year. A recent set of measures with 36-inch telescope shows rapid change in both angle and distance, and renders it certain that this star belongs to the class of binaries with comparatively short periods. It has a proper motion of 0″·201 in the direction of 141°·0. It is obvious that this movement is common to both components, as otherwise the distance in 1891 would be about 3″, with the position-angle about the same as in 1878.

The following are all the measures to this time:—

1878·47	312°·5	0″·60	6·0	9·5	β 2*n*
1879·47	298 ·3	0 ·66	6·5	9·0	β 1*n*
1891·33	247 ·7	1 ·26	6·0	10·3	β 3*n*

This pair is now a very easy object, and should be easily seen with any good refractor of seven or eight inches aperture. It is desirable that it should be measured annually for a few years to get sufficient data for a computation of the period.

Lick Observatory, Oct. 20. S. W. Burnham.

Double Stars for December.

The following interesting double stars are well placed for observation in December. They may all be observed with a telescope of moderate aperture.

O. Struve 50. R.A. $3^h\ 1^m$, Dec. +71° 6′ (1880); Mag. 7·5, 7·5.

A slow-moving binary.

210°·81	1″·05	1886·41	Tarrant.

Struve 367. R.A. $3^h\ 8^m$, Dec. +0° 18′ (1880); Mag. 7·4, 7·6.

229°·4	0″·84	1888·06	Leavenworth.

Struve 422. R.A. $3^h\ 31^m$, Dec. +0° 12′ (1880); Mag. 6, 8·2.

Slow motion.

242°·9	5″·91	1877·0	Plummer.

40 Eridani. R.A. $4^h\ 10^m\ 13^s$, Dec. − 7° 49′·5 (1890).

Triple, 4, 9, 11. 9 and 11 form a binary pair in rapid motion, of which recent measures are:—

111°·03	3″·01	1886·92	Tarrant.
106 ·08	2 ·94	1888·84	Burnham.
105 ·05	2 ·81	1888·87	Tarrant.
100 ·0	2 ·68	1890·73	Burnham.

Struve 535. R.A. $4^h\ 17^m$, Dec. +11° 6′ (1890·0); Mag. 7, 8·3.

333°·8	1″·38	1888·47	Leavenworth.
331 ·75	1 ·87	1888·94	Tarrant (A.N. 2991).

Struve 566 = 2 Camelopardali. R.A. $4^h\ 30^m$, Dec. +53° 15′ (1880). Mag. 5·8, 7·5.

Slow motion in angle. Distance nearly constant.

289°·78	1″·60	1888·03	Tarrant.

Struve 577. R.A. 4^h 34^m, Dec. +37° 17′ (1880); Mag. 7·7, 7·7.

Slow motion in angle. Distance nearly constant.

73°·03	1″·52	1888·96	Tarrant (A.N. 2991).

14 (*i*) Orionis = OΣ 98. R.A. 5^h 2^m, Dec. +8° 21′ (1890); Mag. 6, 6·8.

A binary in retrograde motion. Period 190½ years (Gore).

193°·32	0″·95	1888·1	Schiaparelli.
193 ·4	0 ·98	1888·90	Leavenworth.

J. E. Gore.

Selenographical Notes.

ERATOSTHENES.—This noble ring-plain, which forms so appropriate a termination to the towering escarpment of the Lunar Apennines, is, like the neighbouring Copernicus, more frequently an object which attracts the passive admiration of the desultory observer and his friends than the critical scrutiny of the selenographer. In truth, if viewed at that particular stage of sunrise when the interior is about half illuminated, it presents so many details on its brilliant rampart and beyond, that, in attempting a sketch, it is so difficult to eliminate those delicate lights and subtle gradations of shadow, which are merely the transient effects of sunlight, from other markings which indicate important physical characteristics, that we are often constrained to abandon what appears to be an hopeless task.

Schmidt, writing of Copernicus, says:—" Careful study directed to its unequal beauties and magnificent form is of more value than that devoted to a hundred other craters." With very slight modification the same may be affirmed of Eratosthenes, which is scarcely surpassed by any other lunar ring-plain, excepting by that which has no rival.

Isolated drawings of this formation, doubtless for the reason just given, are rare. The most recent, and certainly one of the most beautiful, was made by that accomplished lunar draughtsman, Dr. L. Weinek of Prague, and is dated 1887, Dec. 23, 6^h to 7^h 15^m. The phase, however, was not favourable for showing details on the floor, the only object visible in the interior being the highest peak of the central mountain-mass, just emerging from darkness into sunlight. In Schmidt's map Eratosthenes has evidently suffered much at the hands of the engraver—salient and important features being masked by the gross prominence given to others of quite minor importance. This is more especially true as regards the great mountain-arm extending from the south-east side of the formation to the west of Stadius.

The best view of the many terraces on the inner slope of the east wall is obtained when the east long. of the morning terminator is about 17°. It is then apparent that Schmidt has not exaggerated their number; for, at times of best definition, in addition to these broad intervening valleys, which cannot be less than two miles across, where they are at their widest, many other concentric grey markings of a more delicate character are visible, which point to the existence of much narrower and shallower features of a similar type. In many instances, both here and elsewhere, as all who use his map well know, he draws these valleys as crater-rows, or rather as rows of craters which, through encroaching one on the other, have lost their individuality as distinct depressions, and exhibit only a scalloped edge on either side of the valley, which is due presumably to their presence. High powers and good atmospheric conditions are needed to decide whether he is justified in representing these valleys as crater-rows so frequently as he does. The fact, however, that so many of the coarser concentric depressions on the ring-plain slopes are clearly of this character, tends to strengthen the belief that some of the more delicate valleys would exhibit the same structure under greater magnification.

On February 17, this year (7^h 30^m to 8^h 30^m), I noted, in addition to a number of valleys, broad and narrow, on the inner slope of the east wall three oval depressions, like large pits, near its foot. They were in a line and were parallel to the edge of the floor. At 8^h, the extreme point of the shadow of the highest peak on the central mountains appeared to touch the foot of the inner slope of the east border. Observers do not agree very closely as to the shape or extent of these mountains, bright and prominent as they are under oblique illumination. Schmidt shows a large round mountain, nearly central, another similar to it, midway between it and the southern side of the floor, a much smaller one north-west of the centre, and a fourth a little north of the centre. Mädler draws a long mountain near the centre, with two small circular hills on the east and a third on the west. Webb speaks of "a fine central hill," and Neison of "three central peaks placed in a triangle." I have observed Eratosthenes very many times during the past twenty-five years, but never, when the central mountains were favourably placed, saw any such arrangement of them as is drawn or described by these observers. Instead of the greater portion of the mountain-mass lying, as Schmidt describes it, south of the centre, I find it on the opposite or northern side. It appears to me thus—two large bright circular mountains on either side of the centre, situated at the southern ends of two slightly curved arms (convex towards the east) in the shape of the letter V, the point of junction of the arms being at no great distance from the inner foot of the northern wall. The peaks, of which there are at least four, are certainly not far from the centre, and the northern section of the mountain is also of considerable altitude and is as bright under a morning sun as the rest. Schröter represents this mountain much in the same way as I see it. About full Moon all that is visible in the interior are five brilliant points of light, three of which are north of the centre.

The most remarkable object beyond the limits of Eratosthenes is the great mountain arm on the south-east, which, extending towards Stadius, ultimately forms the most conspicuous section of the west wall of this very obscure ring. To see this region at its most striking phase, the time should be chosen when the well-known crater-row north-west of Copernicus is just within the morning terminator. The massive double-peaked shadow of the arm then falls beyond the craters, and its southern edge is bordered by crowds of bright little depressions. Among them stands the fine large crater, Stadius B, encircled by a ring of five or six glittering craterlets, which render it one of the most beautiful objects of its class. There is a prominent crater in the *col* between the two peaks on the mountain arm, and another, not shown in the maps, on the eastern plain near its foot, the two objects aligning with Stadius B. THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1891, Nov. 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Varying Lightness of Nights.

GENTLEMEN,—

With reference to Mr. W. F. Denning's remarks in your May number, p. 197, on the varying degree of lightness of the sky at nights, in which he states that during the Perseid period of 1880 the sky was especially light, I may say that this phenomenon must have extended over a large area of country, as I noticed it at the same time, while travelling in Savoy and Switzerland. I presume Mr. Denning's observations were made in England. My observations show the correctness of his statement that the light is not due to any phenomenon low down in the atmosphere, for on the night of August 10, when the light was most remarkable, I was on La Flégère (near Chamonix), altitude 5900 feet. The light was so great that even at midnight Mont Blanc looked perfectly magnificent, every detail being distinct, though the summit is distant nine miles from La Flégère. On that night the sky was nowhere very clear, and there were irregularities in the light, probably owing to thin clouds; but I do not find this always to be the case on light nights, for sometimes they are quite as clear as, if not clearer than, dark nights, even very faint objects, such as the fainter parts of the zodiacal light and small stars, being clearly visible, though of course their distinctness is somewhat hindered by the light.

Mr. Denning does not say anything about the existence of any aurora during the Perseid period of 1880, though I have a note that bright auroras were recorded even in the southern parts of the British Isles on the 11th and 12th.

Yours faithfully,

Sunderland, 1891, Oct. 26. T. W. Backhouse.

The Cause of Variations in Levels.

Gentlemen,—

In your report of the Meeting of the R. A. S. you are unable to give the form of level I proposed. I would therefore refer those interested to a paper in Vol. xxix. p. 302 of the 'Monthly Notices.'

If you would allow me a little space, I should like to say that the main cause of the variations of the length of the bubble of a spirit-level is the expansion of the light volatile fluid in the level whose vapour forms to a great extent the bubble. The volume of this fluid is so great in the present form of level, that but for a portion of the vapour being condensed on its expansion the tube would be burst. The consequence is the inconvenience of short bubbles in summer and excessively long ones in winter, to the very great inconvenience of observers, and the necessity of considering temperature as determining the scale, even when dislevelment is very small.

To mercury as a fluid for the level, it may be objected that the edge against the glass will be rounded and not sharp. That, of course, equally applies to the fluids now used; and every one knows the importance of the mode of lighting the present level-bubbles, and the fact that with change in the mode of lighting serious differences of reading can be made. This can be avoided with mercury by enclosing a small quantity of a fluid in the tube, which shall at once wet both the glass and the mercury; a solution of corrosive sublimate (mercuric chloride) was the medium I proposed to use; unfortunately, I cannot now say where the suggestion came from, but I think it will be found to answer the purpose. A much greater objection to mercury is the risk to brass instruments and graduations on silver or gold. But mercury is not essential. The form of level I propose would much reduce the proportional volume of the fluid, and hence the great variation in the bubble length with temperature, whatever the fluid used. The form of the longitudinal section could possibly be optically tested for truth, at all events it could be made far more truly than now, and the mode of reading the ends could be greatly improved by methods with which I need not now occupy your space.

Yours faithfully,

J. F. Tennant.

Solar Photographic Lenses.

Gentlemen,—

If you can afford the space, will you kindly describe the amplifying lenses or system of lenses used in producing the enlarged photographs of the Sun taken at your observatory and elsewhere *direct in the telescope*? Is a concave amplifier or Barlow lens employed or a positive projector? and are the lenses photographically corrected? In good air are the images of the spots and edge of the Sun satisfactorily *sharp* and well defined? What degree of enlargement is practicable, and what is the size of the magnified image of the Sun taken at Greenwich? What are the largest photographs of the Sun, also of the Moon, taken direct in the telescope by some sort of projecting lens, exclusive of the Lick telescope photographs? Inquirer.

[The magnifier used to enlarge the primary image in the photoheliograph now in use at the Royal Observatory, Greenwich, is a photographic doublet (a double meniscus, the lenses being 2¼ inches apart) of 1·4 inches aperture and 4·2 focal length. The magnification employed is 7·6 times. This gives a sharp image over the greater part of the Sun's disk, but leaves a little to be desired for the limb and centre. As the image in the primary focus is almost exactly one inch in diameter, the resulting enlarged image is about 7½ inches. The largest photographs of the Sun taken direct in the telescope are those taken at Dehra Dûn, India, 12 inches in diameter.—E. W. M.]

Berne Time and the 'Nautical Almanac.'

Gentlemen,—

Yielding neither to General Tennant nor to anyone else in my personal esteem and regard for Dr. Hind, I must beg you, in bare justice, to give me space to state that, had I had the slightest idea that his resignation was so imminent and that he had edited his last 'Nautical Almanac,' my letter in your September number would never have been written. Faithfully yours,

William Noble.

Forest Lodge, Maresfield,
Uckfield, 1891, Nov. 4.

Saturn's Ring.

Gentlemen,—

I enclose a copy of a letter which I quite recently sent to the 'Morning Post':—

"Early this morning, between 5h 30m A.M. and 6h 10m A.M., I saw most distinctly both the ansæ of Saturn's Ring. The colour of them was a vivid though pale blue. The western ansa was brighter than the eastern or following ansa. The ring where it crossed the planet was represented by a dark band rather broader than the western ansa at its junction with Saturn's disk. The north edge of this band was of a strange orange tint. The dark colour of the ring in its passage across the disk of the planet is an effect of contrast. The orange tint is caused by our looking at the disk through the semi-transparent part of the ring. Yesterday morning (Sunday) was so cloudy as to hide every celestial object from view. But on Saturday, October 31, the blue ansæ were certainly not visible in a cloudless sky at $6^h\ 25^m$ A.M., nor were they visible October 30 at $6^h\ 10^m$ A.M., nor on October 29 at $5^h\ 55^m$ A.M. My inference is that the plane of the ring passed through the Sun on the morning of Sunday, November 1, when the planet was hidden by cloud. The line joining the centres of Saturn and the Earth was inclined about 2° 8′ to the plane of Saturn's Ring at 6^h A.M. this morning. My telescope is a refractor by Simms, having 6 in. aperture, with a magnifying-power of 180. The phenomenon of the passage of the ring-plane through the centre of the Sun will not occur again until 1907, July 29."

Yours faithfully,

Murston Rectory, Sittingbourne, Nov. 2. A. FREEMAN.

Note on Bright Transit of Jupiter's Satellite I. and Black Transit of Satellite III.

GENTLEMEN,—

Two observations of transits of Jupiter's Satellites of more than ordinary interest have recently been made here. Since they are perhaps worthy of record, I have pleasure in forwarding them to your valuable journal. During transit on 1891, Sept. 3 (Sept. 2, $22^h\ 17^m$ G.M.T.), a few days before opposition, Satellite I. was visible throughout as a bright spot in contact with its shadows, on the south equatorial belt. It was steadily held, though faint, when central on the disc. 1891, Oct. 9, Satellite III. was seen in transit by Mr. R. T. A. Innes, F.R.A.S., and myself as an intensely black spot. Ingress was not observed, but an hour later the satellite appeared at first glance the most conspicuous marking on the planet, remaining so until twenty minutes before egress, when it faded rapidly and was lost at $10^h\ 5^m$ Sydney M.T. At $10^h\ 8^m$ it became visible as a light spot very near the limb, and at once increased in brightness. Egrees was noted as follows:—Internal contact $10^h\ 9^m\cdot5$. Bisection $10^h\ 13^m\cdot3$. External contact $10^h\ 19^m\cdot3$. Ingress of shadow was recorded:—External contact $10^h\ 10^m\cdot5$. Bisection $10^h\ 13^m\cdot8$. Internal contact $10^h\ 20^m\cdot8$. It will be seen the 'Nautical Almanac' times are somewhat in error. The satellite was centrally situated on the Great Red Spot and was estimated on the central meridian at $8^h\ 40^m$ S.M.T. It was thought, on mentally comparing with the shadow, that the satellite although as black, was certainly not so definite.

The instrument used was an $8\frac{1}{2}$-inch equatorial reflector, the mirrors of exquisite defining-power by With; achromatic eyepieces, powers 180 and 285; definition excellent on both occasions.

Many observations of the surface-markings of Jupiter have been made during the past opposition, which it is my intention to embody in a paper to be read before the Royal Society of N.S.W.

Paddington, N.S.W.
1891, Oct. 12.

Yours faithfully,
WALTER F. GALE.

Meteor and Meteor-streak observed in a Telescope.

GENTLEMEN,—

On Oct. 30 at $9^h\ 13^m$ I was sweeping the sky in the region a few degrees S.W. of α Pegasi, with a power of 40, field $65'$, on my 10-in. reflector, and picked up a pretty faint, large nebula, near some telescopic stars. I had only obtained a momentary glimpse of it, and was in the act of clamping the telescope when both nebula and stars were suddenly obliterated by the light from a fine meteor which shot rapidly across the field from E. to W. It must have been brighter than a 1st mag. star. The nucleus did not present a sharply defined disc, but looked like a molten, flaming mass, expanding sensibly and leaving behind it a bright streak. The end of the meteor was not seen, for it passed beyond the limits of the field before final disruption.

The streak apparently brightened and for an instant it lay like a perfectly straight, luminous bar on the dark sky-ground interspersed with stars. But it immediately afterwards showed great proper motion; the line became undulatory, and one minute after its first projection it exhibited a series of sharp curves, giving it a serpentine form. Selecting the apex of a V-shaped bend, I let it pass through the field, and found its rate of motion to be 1° in 25 seconds, direction from S.S.W. to N.N.E. At first the streak was only $1'$ in width, but in 4 minutes this had increased to $5'$, and it was fainter in proportion. It appeared to be seething and sparkling, as though composed of materials in a state of ebullition, and this effect no doubt belied its true character as a mass of light, floating embers fanned and contorted by wind.

At the time of the observation there was a very strong surface breeze blowing from E.N.E., so that the distant current in which the meteor-streak was borne along must have been travelling in nearly an opposite direction.

The streak probably remained visible 8 or 10 minutes altogether, but after watching it 4 minutes I shifted the telescope to recover the nebulous object referred to at first, but in this attempt I was unsuccessful. Possibly the glare from the meteor may have so contracted the pupil of my eye as to make it incapable of grasping a faint object so soon afterwards. There are several pretty conspicuous nebulæ near α Pegasi (N.G.C. 7448, 7479, and 7515), but these are known to me, and the object I glimpsed on Oct. 30 is not to be identified with either of them.

I may mention that since April 1889 I have observed 228 meteors in my telescope, but the one described above was the brightest of all. On Oct. 29 last I saw a meteor of about 8th mag. 2° W. of β Cygni, and its motion was so slow that it occupied 4 seconds in traversing 33′ of arc. Combustion had no doubt begun some time before the meteor sailed into the field, and its disappearance occurred near the centre. This object, like many others of its class, gave me an impression of its great distance.

Yours faithfully,
Bristol, 1891, Nov. 5. W. F. Denning.

A New Belt on Jupiter.

Gentlemen,—

Much has been published lately on the markings on Jupiter, principally on his belts, the great red spot, and on the small black and white spots, &c., &c., but I have seen nothing confirmatory or otherwise regarding an observation made by me on the morning of Sept. 3, 1891.

If my memory is not at fault, no belt has ever been seen exactly on Jupiter's equator, the two usually thus called being one on each side, therefore, strictly speaking, are not equatorial.

On the above date I saw what I had never before seen—a belt exactly between the two equatorial belts, and, consequently precisely on his equator. It was first glimpsed with a power of 132 on my 16-inch refractor as an exceedingly delicate dark marking extending from limb to limb, but quite irregular and apparently discontinuous in places; but with powers of 200 and 360, the seeing being good, it was better seen and with the continuity unbroken. Its general trend was parallel with the others, and, of course, with the equator, but its outline was quite irregular, being only a line in some places and belt-like in others.

On Sept. 24, with seeing good, it was again carefully looked for, but not a vestige of it could be seen. Since then, with seeing 3 on a scale of 5, it has not been re-observed.

Has any astronomer seen a like phenomenon?

Warner Observatory, Rochester, N.Y. Yours truly,
1891, Nov. 9. Lewis Swift.

Prediction and Non-Prediction.

Gentlemen,—

One of the facts most generally known in the history of astronomy is that the first successful prediction of the return of a comet was made by Halley in the case of that which was afterwards called by his name. The grounds of his conclusion are, however, not always accurately stated, some fancying that he calculated the period of the comet of 1682 observed by himself and others.

What he did was to compare the elements of the orbits of all the comets which had been in his time sufficiently observed (and of which the observations were accessible to him) to give hopes that these elements might be considered approximately accurate.

When he had done so, he was struck with the similarity between the orbits as calculated of the comets which appeared in the years 1531, 1607, and 1662, and of those of 1532 and 1661. He suggested, therefore, identity in both these cases; but whilst he did so boldly in the former case and confidently predicted the comet's return in 1758, he expressed himself doubtfully in the latter in these words:—"Crediderim equidem Cometam etiam anni 1532, eundem fuisse cum illo, qui ab *Hevelio* observabatur ineunte anno 1661; sed observationes *Apiani*, quas solas de primo habemus, nimis rudes sunt, nec quicquam certi in re tam subtili ex iisdem elici potest."

In the case of the comet now always known as Halley's by reason of his confident prediction of its return, he had not only accurate observations at two appearances, in 1607 and 1682, upon which to found his conclusion, but there were the additional confirmations that a similar orbit resulted from Apian's rough observations in 1531 and that a large comet had been seen (though not astronomically observed) in the summer of 1456, "eodem pene modo inter Solem et Terram transiens retrograde," so that the periods between the four appearances nearly corresponded. With regard to the appearance in 1607, Halley was acquainted with the observations of Kepler at Prague and Longomontanus at Copenhagen.

Nearly eighty years after his paper ("Astronomiæ Cometicæ Synopsis") was published (it first appeared in the 'Philosophical Transactions' for 1705) the observations by Harriot in England and by Lower in Wales were brought to light by Von Zach.

Respecting those of Lower, I recently made a communication to the 'Observatory.' It is much to be regretted that the manuscripts did not fall first into the hands of an English astronomer, Von Zach making many mistakes in their decipherment which were afterwards corrected by Rigaud. One of these, which I omitted to note in my last letter, was the attribution of Lower's observations in Carmarthenshire to Nathaniel Torporley. Mistakes, even when corrected, often reappear, and this is unfortunately followed in Carl's valuable 'Repertorium der Cometen-Astronomie.'

Notwithstanding the grave doubts felt by Halley with regard to the conjectured identity of the comets of 1532 and 1661, Maskelyne wrote a paper in the 'Philosophical Transactions' for 1786 with the object of facilitating a search for it in 1788. The French Academy had, he remarked, proposed the discussion of its motions as the subject of a prize, which had not then met with a satisfactory response. Méchain, however, not long afterwards undertook the investigation, with the result that the identity of the two comets was not probable. The elements of both have some similarity, as Valz pointed out, with those of the periodical comet of Brorsen; but the supposition of identity would imply a very great change in the comet's personal appearance. Yours faithfully,

Blackheath, 1891, Oct. 9. W. T. LYNN.

Early Observations of *Saturn's Rings by Wright, of Durham.*

Gentlemen,—

In discussions respecting early perceptions of an appearance which was doubtless produced by the dusky ring of Saturn, an observation by Thomas Wright, of Durham (whose name is chiefly known by its connexion with the so-called island theory of nebulæ), has, it appears to me, been overlooked. It is found in his 'Clavis Cœlestis,' published in 1742, where he says, p. 34:—

"Thus far I have describ'd this Ring, as supposing it form'd of one united Piece of Matter; but thro' the best Telescopes, it appears to be made up of many Rings, of which two are very visible, and a third perceptible. These I saw thro' a five Foot Newtonian Reflector (made by Mr. Jackson) 1739, in *March*; the outward one being to the inward one as 1 to 3, but the rest were very obscure. At this Time the Ring appear'd most open."

This is surely the first occasion on which the dusky part is spoken of as a separate or "third" ring.

It is in p. 65 of his other work, 'An Original Theory or New Hypothesis of the Universe' (1750), that Wright expresses the opinion that "could we view *Saturn* thro' a Telescope capable of it, we should find his Rings no other than an infinite Number of lesser Planets, inferior to those we call his Satellites."

This view had indeed been suggested before, but not so definitely as here by Wright, who assigns as his reason for believing it that the "Ring or Collection of small Bodies, appears to be sometimes very excentric, that is, more distant from *Saturn's* Body on one side than on the other, and as visibly leaving a larger Space between the Body and the Ring."

Yours faithfully,
W. T. Lynn.

Blackheath, 1892, Oct. 20.

The Solar Eclipse of B.C. 217.

Gentlemen,—

I have not seen any reference in modern historians (or indeed since Riccioli) to this eclipse, which occurred in the second year of Hannibal's campaigns in Italy, not long before his victory over Flaminius at the lake Trasimenus. Speaking of the prodigies which, according to Livy (lib. xxii. c. 1), were manifested about the beginning of that year, one was that it was noticed (apparently [illegible]) that "solis orbem minui visum." At Arpi the historian goes on to speak of "pugnantem cum luna solem," and at Capua "duas interdiu lunas ortas." The first of these two (the [illegible]) evidently has respect to a meteorological phenomenon [illegible] to the eclipse; but there can be little doubt that the [illegible] before, about the diminution of the Sun's orb, [illegible] The eclipse took place on the 11th of February, and was [illegible] Africa in the afternoon of that day. According [illegible] 'Canon der Finsternisse,' Eclipse,—416, ii. 11) the e of centrality, after crossing the Atlantic from the northern rt of South America, entered Africa near Cape Verde, and traersing that continent in a north-easterly direction, ended in the Mediterranean between the islands of Crete and Cyprus. It was doubtless visible as a large partial eclipse in Southern Europe, and the mention by Livy (whose record is copied by Julius Obsequens) forms an additional confirmation (besides that of the eclipse in the year of the battle of Zama) of the accepted dates of the events in the Second Punic or Hannibalic War.

Yours faithfully,

Blackheath, 1891, Oct. 28. W. T. LYNN.

PUBLICATIONS.

SYDNEY DOUBLE-STAR MEASURES *.—This is a reprint of a paper submitted to the Royal Astronomical Society in March 1891. It embodies the measures made at Sydney from 1882 to 1890, with the addition of previous observations by other observers where motion is suspected. The pamphlet certainly brings forward the fact that southern doubles are much neglected, and we can only hope that more attention will be given to this class of observatory work in future. There are stars in this list well worthy of persistent attention.

STAR GROUPS †.—This is a neat and handsome compilation by Mr. J. E. Gore. The author has evidently aimed at presenting the different groups of stars, to very young beginners, in as attractive and instructive form as possible. We think he has succeeded.

PRACTICAL ASTRONOMY AS APPLIED TO LAND SURVEYING ‡.—Mr. Downes has attempted to put down in 20 pages all the astronomy that land-surveyors need know, to save them the trouble of wading through text-books. As in similar cases some of his remarks (notably those about time) would convey erroneous ideas to an astronomer, though they may, and probably will, convey correct ones to a land-surveyor. The style is concise and simple.

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* Results of Double-Star Measures made at Sydney Observatory under the direction of H. C. Russell, B.A., F.R.A.S. &c. (Turner & Henderson, Sydney, 1891.)

† Star Groups; a Student's Guide to the Constellations. By J. E. Gore. (London: Crosby Lockwood & Son, 1891.)

‡ Practical Astronomy as applied to Land-Surveying, by R. H. B. Downes — Proc. of Inst. Civil Eng. Published by the Institution.

NOTES.

THE GEGENSCHEIN.—In No. 243 of the 'Astr. Journal' Prof. Barnard summarizes his observations of the Zodiacal Counterglow or Gegenschein during the years 1888–1891. These were made under better conditions than those by the same observer at Nashville in 1883 to 1887 (Astr. Journ. vii. p. 186) and satisfactorily confirm them.

"When first seen in the fall, the Gegenschein is large and roundish, and does not seem to be connected with any zodiacal bands. Later, after passing 0^h, it becomes very much elongated along the ecliptic, and is connected with the evening and morning zodiacal light by a narrow zodiacal band . . . usually from 3° to 4° wide, and lying along the ecliptic."

Mr. Barnard's observations of position, 16 in number, "made with the greatest care and which should have great weight in any discussion of the phenomenon," give for the position (λ=longitude, β=latitude)

$$\odot-\lambda=180^\circ\cdot6, \qquad \beta=+1^\circ\cdot3.$$

His previous observations (1883–1887) gave

$$\odot-\lambda=180^\circ\cdot6, \qquad \beta=+0^\circ\cdot3.$$

These "prove conclusively that the phenomenon is nearly always seen in a north latitude. It is possible that this may in part be due to atmospheric absorption, though the observations, extending through 40° of zenith-distance, do not give much weight to this suggestion. It is a question, however, that can only be settled by corresponding observations in the southern hemisphere. Parallax would tend to give it a southern latitude.

"The observations also prove that the *Gegenschein* is not always exactly 180° from the Sun. There is certainly an oscillation of a degree or more from $\odot+180^\circ$. That is, $\odot-\lambda$ is not a constant quantity, there being a tendency to a less longitude than $\odot+180^\circ$.

"In some cases the observations have been made three or four hours from the meridian, but they do not seem to show any positive parallax—on the assumption that $\odot-\lambda$ is a constant quantity.

"I have suggested in my previous paper (A. J. vii. p. 186) that the phenomenon may be due to some abnormal condition of refraction by our own atmosphere. There are a number of objections to this theory, but it might be well to consider it further.

"In his *Mechanical Theory of the Corona*, as published in the Lick Observatory Report of the eclipse of 1889 Dec. 21–22, Professor Schaeberle has pointed out that a phenomenon similar to the observed *Gegenschein* will be produced by a system of nearly radial luminous lines having the Sun as a centre of divergence. The centre of the *Gegenschein* is one vanishing point of such a system of lines, while the centre of the solar corona is the nearly diametrically opposite vanishing point of a similar system of lines.

"These lines, he claims, produce in perspective all the well-known coronal forms. Perhaps sufficient observations have now accumulated to test one or the other of these theories."

ELIZABETH THOMPSON SCIENCE FUND.—This fund, which has been established by Mrs. Elizabeth Thompson, of Stamford, Connecticut, "for the advancement and prosecution of scientific research in its broadest sense," now amounts to 26,000 dollars. As accumulated income will be available December next, the trustees desire to receive applications for appropriations in aid of scientific work. This endowment is not for the benefit of any one department of science, but it is the intention of the trustees to give the preference to those investigations which cannot otherwise be provided for, which have for their object the advancement of human knowledge or the benefit of mankind in general, rather than to researches directed to the solution of questions of merely local importance.

Applications for assistance from this fund, in order to receive consideration, must be accompanied by full information, especially in regard to the following points:—

1. Precise amount required. Applicants are reminded that one dollar is approximately equivalent to four English shillings, four German marks, five French francs, or five Italian lire.
2. Exact nature of the investigation proposed.
3. Conditions under which the research is to be prosecuted.
4. Manner in which the appropriation asked for is to be expended.

All applications should reach, before December 10, 1891, the Secretary of the Board of Trustees, Dr. C. S. Minot, Harvard Medical School, Boston, Mass., U.S.A.

It is intended to make new grants at the end of 1891.

The trustees are disinclined, for the present, to make any grant exceeding three hundred dollars; decided preference will be given to applicatious for smaller amounts.

THE SOLAR PARALLAX FROM OCCULTATIONS. — The peculiar advantages of occultations for determining the Moon's place are too well known to need a detailed explanation. The instantaneous disappearance of the star is significant of the absence of a sensible lunar atmosphere; and the real limb of the Moon is known to pass through a very definite point at this moment. The real limb may be said to be unknown to the meridian or altazimuth observer. He is accustomed to record the transit of a limb made indefinite by diffraction phenomena, and of which different observers select different zones for observation; and, what is more important, the same observer will select different zones according to the age of the Moon, for observations near new Moon are necessarily made in a daylight sky when the fainter zones of the diffraction border are lost; while they show quite strongly against the night sky of full Moon. Thus meridian observations near new Moon are not comparable with those at full Moon. Again, meridian observations are made on the bright limb, which is the preceding limb before, and the following limb after, full Moon. To obtain the place of the centre we must add the semidiameter in the first case and subtract it in the second, and thus, if there is any doubt about the diameter, observations before and after full Moon are not comparable. There are thus considerable difficulties about determining the parallactic inequality, whose period is a month, from meridian observations. The coefficient of this inequality contains the solar parallax as a factor, and a good determination of the coefficient would give us a good value for the solar parallax. But a discussion of meridian observations for such a purpose is more than likely to give us quite a fictitious parallactic inequality, depending on the changes of personality in observation of the limb. Now the information given by occultations is free from this defect. We are here dealing with the real limb, and the only personality which concerns us is the difference between that for disappearances and reappearances, which enters into the diameter. There is, however, the drawback that the number of stars in the path of the Moon bright enough to be satisfactorily observed at occultation is small, and the number which can be observed at both disappearance and reappearance is smaller still. The near neighbourhood of the bright Moon overpowers the light of the star and tires the eye.

It was pointed out some years ago that an exceptional opportunity is presented by a total eclipse of the Moon. Here *both limbs are dark*, and there is no moonlight to overpower faint stars. We thus have an opportunity of observing the disappearance and reappearance of the *same star*, even though it be 9th or 10th magnitude, of which there are sure to be some occulted. Unfortunately the eclipses of 1884 Oct. 4, and 1889 Jan. 28, have shown that the probable error of such observations is much larger than was expected from similar observations of bright stars when there was no eclipse.

Thus it cannot yet be said that we have sufficient material for determining the occultation-diameter and the parallactic inequality.

Dr. Battermann * has made a gallant attempt to supply this deficiency, or at least to show how it may be supplied. He has shown that near new Moon the occultation of comparatively faint stars can be observed; and hence the number of available occultations largely increased. Such occultations are not at present predicted by computation in our almanacs. It is usual to compute those for stars down to 6½ magnitude only; but it would be well to omit some of these near full Moon, and to add fainter ones near new Moon. In default of computed predictions, Dr. Battermann used a graphical method for predicting reappearances, and simply watched for the disappearances. The result is a fine list of 250 occultations from 1884 April to 1885 October, during which time the Greenwich observers, using the list of the N.A., only obtained 26.

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* "Beiträge zur Bestimmung der Mondbewegung und der Sonnenparallaxe aus Beobachtungen von Sternbedeckungen am sechsfüssigen Merz'schen Fernrohr der Berliner Sternwarte," von Dr. H. Battermann, Beobachtungs-Ergebnisse der k. S. zu Berlin, Hefte No. 5.

The deduced places of the Moon are carefully discussed for parallactic inequality, and the resulting solar parallax is

$$\pi = 8''\cdot794 \pm 0''016.$$

MAGNETIC OBSERVATIONS IN NOVA ZEMBLA [*].—The first part of the results obtained by the Russian Polar Expedition in the winter 1882–3 has just reached us. The most interesting part of it is the brief account of the expedition itself in the Introduction. The worst trials of the explorers seem to have been the snow showers. "On the 12th of November the Sun was visible for the last time, and now began the 3 months of true Polar night. With the end of November came severe snowstorms, impossible to describe to those who have not witnessed them. The first signs of a coming storm are milk-white clouds in the eastern horizon; a S.E. wind gets up which rapidly increases in about an hour to a tempest, and remains furious for days. On the first and second days of the storm thick snow and a number of small stones are blown from the neighbouring mountains, and beat on the roofs of the houses with such noise as to drown all ordinary conversation; the rattle is like that of looms in a big factory. In the garrets and outhouses snow falls as thickly as in the open. On such days it was very difficult to make observations. The observers could scarcely take the few steps from one instrument to another. They generally went slowly backwards, holding tightly to a rope; the snow was blinding, and the wind made it almost impossible to breathe." In such conditions the ordinary observations of magnetic declination, dip, and horizontal force were made with regularity!

COMET NOTES.—No. 3064 of the Ast. Nach. contains a discussion by Dr. Oppenheim of the orbit of Brooks' Comet (1886 IV.). The most probable period is 5·5954 years, which would make the next perihelion passage 1892, Jan. 13. He thinks there is little prospect of seeing the comet at the next perihelion unless it should occur considerably later than this. He gives ephemerides on the assumption that perihelion takes place—(1) March 1, (2) March 31, (3) April 30, (4) May 30, (5) June 29, (6) July 29.

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[*] "Beobachtungen der Russischen Polarstation auf Nowaja Semlja."

Wolf's Comet promises to be within reach for some months longer. An Edinburgh circular has been issued giving an ephemeris by Berberich up to April 3, 1892, when the brightness will be half that at rediscovery. It reaches its greatest south declination on Dec. 19, and by April 3 will be again near the equator. We append an ephemeris for December:—

	R.A.	Decl.		R.A.	Decl.
	h m s	° ′		h m s	° ′
Dec. 3..	4 22 58	−13 42·2	Dec. 19..	4 15 41	−14 51·2
7..	4 20 40	14 14·5	23..	4 14 49	14 46·3
11..	4 18 39	14 36·2	27..	4 14 23	14 34·3
15..	4 16 59	−14 48·1	31..	4 14 24	−14 16·3

The places are for Berlin Mean Midnight.

Tempel-Swift's Comet was observed at Kiev on Oct. 24 in R.A. $21^{\mathrm{h}}\ 8^{\mathrm{m}}\ 57^{\mathrm{s}}{\cdot}14$, N. Dec. $4^{\circ}\ 19'\ 15''{\cdot}6$, the G.M.T. of observation being $7^{\mathrm{h}}\ 18^{\mathrm{m}}\ 18^{\mathrm{s}}$.

Ephemeris for December.

	R.A.	N. Decl.		R.A.	N. Decl.
	h m s	° ′		h m s	° ′
Dec. 1..	23 42 1	21 33·9	Dec. 13..	1 9 13	26 14·9
5..	0 9 57	23 26·2	17..	1 38 46	27 3·7
9..	0 39 18	25 1·4	21..	2 7 3	27 26·8

The brightness reached a maximum on Nov. 27, when it was 3·5 times that at rediscovery.

The Libration of Hyperion.—The third of Dr. Hermann Struve's papers on Saturn's Satellites appears in Ast. Nach. No. 3060. The former two dealt with Rhea and Tethys ('Observatory,' No. 159, p. 94) and with Mimas and Enceladus (No. 167, p. 330); and belong to the first of the investigations which Dr. Struve proposed to undertake, viz. the determination of the orbits of the five inner satellites. He is now attacking the second problem—that of Hyperion; and a third is yet to come—the consideration of the ball and ring system. In the present paper a libration in the longitude of Hyperion is discussed. A few observations only are selected for this purpose, but they are sufficient to give clear evidence of the existence of this term, whose period is 641 days. If we put

$$V = 4l - 3l_1 - \omega,$$

where l and l_1 are the mean longitudes of Hyperion and Titan respectively, and ω the mean longitude of the perisaturnium of Hyperion, then owing to the relation

$$4n-3n_1=\Delta\omega$$

between the corresponding mean motions, V oscillates about a mean value (180°), and it is this oscillation which is called a libration. There will be corresponding inequalities in the longitudes of Hyperion and Titan, of amplitudes inversely as the masses. In the longitude of Hyperion this inequality would be, neglecting the mass of Hyperion as compared with Titan,

$$\delta l=9^\circ \sin 0^\circ{\cdot}562\,(t-t_0),$$

and the application of this term is found to distinctly improve the observed longitudes of Hyperion. The term in Titan will be small, depending on the mass of Hyperion; but if it can be detected it will give us a value for the mass of Hyperion, or at least a superior limit to its value.

A Classification of Periodic Comets.—It will be remembered that Prof. Barnard followed Comet 1889 I. to a distance greatly exceeding previous records, and greater than the aphelion distance of many periodic comets. This opens up possibilities of following comets throughout the whole of their orbits; but in a recent communication to the 'Astronomical Journal' (No. 246) Prof. Barnard is careful to point out an important limitation of such possibilities. Comet 1889 I. was of a peculiar physical appearance, resembling some of the periodic comets, but not all. There are in fact, according to his experience, two distinct classes of periodic comets, if not three. Those in the class of which Comets 1889 I. and 1889 II. are an "exact counterpart" (except in being periodic) are "comparatively small, and have an indefinite central brightness or nucleus, and often an incipient bushy tail. Their principal characteristics are their smallness and indefinite nuclei. These peculiarities are more pronounced when the comet is coming first into view or is disappearing. Many of the parabolic comets resemble them, and there are hundreds and perhaps thousands of nebulæ just like them in appearance. . . . That some one of these comets will be followed completely around its orbit some day is only a matter of a reasonable increase in our present telescopic power." Another class of periodic comets, which he calls Class I., are "large, round, and very gradually brighter in the middle, with no special condensation, and of a very diffused nature. They are from one to several minutes in diameter, and have no nucleus or tail. These are distinctly periodic. Indeed, I have seen no representative of this class that was not periodic. . . . Trusting to this peculiarity, I predicted, at the first observation, that the comet discovered by Swift, 1889 Nov. 16, was of short period (see A. J. 207). A similar prediction was made at the first observation of D'Arrest's Comet in 1890, when I suppose the discovery was an entirely new comet (see A. J. 227)."

THE GALE OF NOVEMBER 11.—The following are some particulars of this gale as observed at the Royal Observatory, Greenwich. The wind began to increase in strength shortly before midnight on November 10, the direction being S.S.E. During the night it further increased, the greatest pressure registered up to $7^h 45^m$ A.M. of November 11 being 5 lb. on the square foot. The pressure record was lost from $7^h 45^m$ to $9^h 15^m$ A.M. At 10^h the direction was S.S.W., and from this time until 11^h pressures considerably above 5 lb. were many times registered, the greatest being $10\frac{1}{2}$ lb. There was then some cessation, until shortly after $1^h 20^m$ P.M., at which time the force of the wind increased in the most sudden manner, the pressures, which had been ranging from 2 to 4 lb., going suddenly up to pressures ranging from 10 to 15 lb. This increased force was maintained until 4^h P.M.; the force then gradually declined until midnight. The greatest pressures registered at the height of the gale were between 1^h and 2^h P.M. =25 lb., between 2^h and 3^h $=31\frac{1}{2}$ lb., between 3^h and 4^h =25 lb., and between 4^h and 5^h =13 lb. During these four hours pressures above 10 lb. were very many times recorded, above 15 lb. 11 times, above 20 lb. 4 times, the greatest, $31\frac{1}{2}$ lb., occurring shortly after $2^h 30^m$ P.M. The wind, which at 10^h A.M. was S.S.W., gradually veered to W.S.W. by 3^h P.M., at which point it remained until midnight, becoming afterwards S.W. There was no change of direction with the sudden increase of force at $1^h 20^m$ P.M. The velocity of the wind at midnight of November 10 was 17 miles per hour, slightly increasing towards morning, being between 7^h and 9^h, 22 miles, between 9^h and 11^h, 25 to 30 miles. Then a lull, as before mentioned. Between 1^h and 5^h P.M., the hourly velocity was generally nearly 40 miles per hour, being at $1\frac{1}{2}^h$ for a period of about 7 minutes as much as 50 miles. [These velocities have been corrected for instrumental error as kindly determined by Mr. Dines.] The barometer, which at 8^h P.M. on November 10 read 29·29 in., had descended by 5^h A.M. following to 28·63 in., and by $11\frac{1}{2}$ A.M. to 28·31 in., a fall of 0·98 in. in $15\frac{1}{2}$ hours. The reading then increased to 28·59 in. by 3^h P.M. on November 11 and to 29·06 in. by 9 P.M., a rise of 0·75 in. in $10\frac{1}{2}$ hours. Rain commenced to fall on the evening of November 10, and fell nearly continuously until about 3^h P.M. on November 11, amount =0·90 in.

It is believed that the record of occasional large pressures of wind previous to the year 1882 was due principally to irregular action in excessive gusts of the copper wire connecting the pressure-plate with the registering-pencil. For this wire a brass chain was substituted in the year mentioned, which, always in tension, appears to have much improved the record. Since the year 1882 no pressure equal to that of November 11 has been recorded, the greatest in each year having been, as follows:—in 1882 (after introduction of brass chain) 29 lb., in 1883 28½ lb., in 1884 23 lb., also in 1885, in 1886 28 lb., in 1887 20 lb., in 1888 21 lb., in 1889 5 lb., also in 1890, and in 1891 (November 11) 31½ lb.

THE SOLAR PARALLAX FROM HELIOMETER OBSERVATIONS AT TRANSITS OF VENUS.—Dr. Auwers has published * the results of heliometer measures of the distance of Venus from the Sun's limb at the following stations:—

1874	Tschifu	96	measures.
	Kerguelen Is.	65	„
	Auckland Is.	97	„
	Mauritius	50	„
1882	Hartford	128	„
	Aiken	48	„
	Bahia Blanca	112	„
	Punta Arenas	158	„

The result from the 1874 transit is 8″·877 ± 0″·043, and from the 1882 transit, 8″·879 + 0″·037. The use of a parallax of 8″·80 increases the sum of 18 residuals for the 1874 transit from 4″·77 to 5″·05; and of 27 residuals for the 1882 transit from 5″·62 to 6″·12.

THE LUNAR ECLIPSE OF 1891, Nov. 15.—Observations of this eclipse seem to have been almost universally frustrated. Nothing was done in the British Isles (Greenwich, Clapham, Ealing, Stonyhurst, Bermerside, Dublin, Glasgow, &c.), or at Paris, Brussels, Dresden, Genf, Hamburg, Strassburg, Utrecht, Kiel, Bothkamp, or New York. It was clear at Halifax in Canada, and at Madrid, Lisbon, and Bermuda. It is perhaps fortunate that the list of stars to be occulted during the total phase was comparatively short and unimportant.

LES ÉTOILES FILANTES DE NOVEMBRE.—La forte bourrasque qui a sévi, du 12 au 16 novembre, au nord-ouest de l'Europe, a tout bouleversé dans nos contrées et dans les stations de notre société. Aussi les observations de météores lumineux durant les trois nuits des 13–14, 14–15, et 15–16 courant, ont échoué et n'ont rien pu révélér sur la pluie météorique de cette année.

Grâce à quelques éclaircies parmi les nuages et la brume, on a pu çà et là reconnaître que le flux météorique était cette année plus abondant que les années précédentes.

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* Astron. Nach. No. 3066.

Ainsi à Pavie, dans la nuit du 14–15, on a observé onze météores, presque tous splendides, émanant de la constellation du Lion.

A Moncalieri, dans la nuit du 15–16, dix autres Léonides des plus brillantes se sont révélées.

A Rome, dans la nuit de 13–14, on a pu en reconnaître 35, et 11 dans la nuit du 15–16. Quelques-unes à peine ont été observées à la forteresse d'Altare, près de Savone, et à San Martino in Pensili (Campobasso); rien ailleurs.

Il est regrettable qu'on n'ait pu suivre cette année la progression du phénomène et constater d'une façon plus sûre la fréquence des réapparitions. P. F. Denza.

De l'Observatoire de Moncalieri, le 23 novembre, 1891.

M. Palmieri, Director of the Vesuvian Observatory, is responsible for the statement that all the great eruptions of Vesuvius take place at new or full Moon, and especially at Eclipses. The eclipse of June 17 was also accompanied by violent earth-currents.

On the other hand we learn that "Captain de Montessus, who has patiently accumulated observations and data concerning earthquakes, has now a Catalogue of more than 60,000 of these phenomena, individually discussed. He establishes that earthquakes are distributed uniformly throughout day and night; that they have no relation to Moon culminations and astronomical seasons, and that such coincidences which have been claimed in the past rest on insufficient grounds" (Bull. Astr. tome viii. p. 513; the original paper being in 'Archives de Genève,' t. xxii. et xxv. 1889 et 1891).

Mr. S. C. Chandler, who has done such good work on Variable Stars, has temporarily transferred his attention to the latitude, which may be called a suspected Variable. The first of what is obviously to be a series of papers on this subject appears in Ast. Journ. No. 248, and deals with the evidence of the Almucantar, which is distinctly *for* variability. We defer summary or criticism until the series of papers is complete.

The second volume (R.A. 6^h to 12^h) of the Great Paris Catalogue was presented to the Academy of Sciences on September 21, and will doubtless soon be distributed. It may be recalled to mind that this Catalogue is founded on about 500,000 observations made at Paris during the last 50 years of the 48,000 Lalande stars. The first volume was distributed in 1888 ('Observatory,' No. 138, p. 303).

The attempt to find solid rock near the summit of M. Blanc, by boring galleries through the ice, having been so far unsuccessful, M. Janssen is considering the feasibility of founding an observatory on the ice.

JUL 21 1939